Working Paper Series

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Monetary policy transmission over the leverage cycle: evidence for the euro area

No 2421 / June 2020

Abstract

We study state dependence in the impact of monetary policy shocks over the leverage cycle for a panel of 10 euro area countries. We use a Bayesian Threshold Panel SVAR with regime classifications based on credit and house prices cycles. We find that monetary policy shocks trigger a smaller response of GDP, but a larger response of inflation during low states of the cycle. The shift in the inflation-output trade-off may result from higher macro-economic uncertainty in low leverage states. For an alternative regime classification based on turning points we find larger effects on GDP during contractions.

Non-technical summary

We study state dependence in the effects of monetary policy over the leverage cycle for a panel of 10 euro area countries over 1982 to 2017. We extract medium-term fluctuations in credit volumes and in real house prices from univariate filtering methods and define country-specific regimes of high and low states in the cycles. We then estimate the effects of monetary policy shocks on GDP, inflation, and the short-term interest rate separately for the two regimes from a Bayesian structural panel VAR.

We find that the impact of monetary policy shocks on GDP is smaller and less persistent during low states of the leverage cycle than during high states. By contrast, the response of consumer prices is larger. While we restrict our main estimates to the period before the 2008 Financial Crisis to avoid issues related to the zero lower bound, these results carry over to estimates covering the period until 2017.

The effect on output can be explained from a stronger credit channel of monetary policy in periods of high leverage, as banks experience a stronger shift in their net worth after a monetary policy shock and therefore adjust their lending more strongly. The shift in the inflation-output tradeoff may be explained from higher macroeconomic uncertainty during periods of low leverage, resulting in higher price flexibility and a quicker pass-through of monetary policy to inflation. We indeed find larger forecast errors for GDP and inflation during low states indicating higher uncertainty.

We also consider an alternative regime classification into expansions and contractions based on turning points in the cycles. We find some evidence for stronger effects of monetary policy shocks on GDP during contractions, possibly related to the role of monetary policy in easing collateral constraints. This suggests that monetary policy remains effective at the onset of financial crises, but has particularly weak effects on output at the early stages of recovery from leverage cycle troughs.

The low efficiency of monetary policy in restoring output during recoveries from leverage cycle troughs strengthens the case for 'leaning against the wind' strategies to avoid large cyclical fluctuations in credit and house prices in the first place.

1 Introduction

The sluggish recovery from the 2008 Financial Crisis in advanced economies has spurred a debate about the role of financial conditions in the transmission of monetary policy shocks. Central banks, in particular, have stressed that transmission mechanisms have been impaired by financial frictions and high uncertainty.[1] In this paper, we study state dependence in monetary policy transmission over the leverage cycle for a panel of 10 euro area economies in between 1982 and 2017. We define country-specific regimes in the medium-term fluctuations of credit volumes and of real house prices and compare the impact of policy shocks across regimes from a Bayesian structural panel VAR for GDP, inflation, and the short-term interest rate.

Various theoretical literature suggests that monetary policy transmission is sensitive to the state of credit and house price cycles, but it comes up with diverse conclusions on the nature and direction of state dependencies. De Groot (2014) argues that highly leveraged banks experience a stronger shift in their net worth after a monetary policy shock and therefore adjust their lending more strongly. Studies based on bank-level data indeed find this outcome (Kishan and Opiela, 2000; Kashyap and Stein, 2000). This mechanism amplifies the effects of monetary policy shocks in states of high leverage. Two recent papers focus on household balance sheet repair and the role of collateral constraints. Alpanda and Zubairy (2019) argue that monetary policy is less effective if debt levels are high, as household borrowing capacity is limited by collateral constraints. Harding and Klein (2018) and Jaccard (2020) yet contend that the effects of monetary policy are amplified in periods of deleveraging as it acts to shift these collateral constraints, whereas initial debt levels play less of a role.

Some of these studies also provide respective empirical evidence for their conclusions. For the U.S., Alpanda and Zubairy (2019) find weaker effects of monetary policy on output in case of high levels of the household debt gap, while Harding and Klein (2018) report larger effects during periods of deleveraging. Alpanda, Granziera, and Zubairy (2020) extend the analysis to a panel of 18 countries and again find smaller effects for a high household debt gap, but they consider only the upper tails of the cycle and

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[1] See, for instance, the widely recognized speech by ECB president Draghi on 26 July, 2012.

the results are not robust against excluding the period after 2006. By contrast, based on annual historical data for a panel of 17 countries, Jordà, Schularick, and Taylor (2019) find larger effects of monetary policy on output for high credit-to-GDP gaps.[2]

In our contribution we study the role of credit and house price cycles for the euro area. While earlier studies have focused on credit gaps, house price cycles are of interest as well, as they are closely aligned with credit gaps reflecting the interplay of credit and the value of collateral, termed the leverage cycle by Geanakoplos (2012). We estimate medium-term fluctuations in credit volumes and in real house prices from one-sided univariate filters. We then define states of high and low leverage from the level of the estimated cycles. Alternatively, we consider a regime classification into expansions and contractions based on turning points in the cycles.

Compared to the local projections methods used in the above studies, our Bayesian Threshold Panel SVAR based on Canova (2005) and Gambacorta, Hoffmann and Peersman (2014) allows for a more comprehensive analysis along various lines. First, our identification scheme, which combines sign and magnitude restrictions, ensures that our estimates satisfy conventional assumptions about the effects of monetary policy shocks in both regimes. This should provide more rigorous findings on potential state dependencies. In particular, we avoid the price puzzle that arises in some of the above studies, as they rely on recursive identification. Second, the SVAR enables us to explore state dependencies with respect to other types of shocks and to study shifts in uncertainty across regimes from an in-sample forecasting exercise. Third, our Bayesian approach provides confidence bounds for the differences in impulse responses across regimes, while partial shrinkage in parameter estimates across countries is more robust against cross-sectional dependence than panel regressions.

We find evidence for both level and momentum effects. As to the former, the impact of monetary policy shocks on GDP turns out smaller and less persistent during low leverage states than high leverage states. By contrast, the response of consumer

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[2]Earlier literature on state dependence in U.S. monetary policy transmission relates to business cycles and financial market uncertainty. Weise (1999) and Lo and Piger (2005) report larger effects of monetary policy shocks on output and inflation during recessions, but Tenreyro and Thwaites (2016) find the opposite outcome. For the euro area, Peersman and Smets (2002) also find larger effects during recessions. Aastveit, Natvik and Sergio (2013) and Eickmeier, Metiu and Prieto (2016) report smaller effects on output for high volatility regimes of the S&P 500.

prices tends to be larger. In states of low leverage, the GDP response lacks the usual hump-shaped pattern and GDP returns to baseline after about 12 quarters. For high leverage, we find a hump-shaped response. While we restrict our main estimates to the period before the 2008 Financial Crisis to avoid issues related to the zero lower bound, these results carry over to estimates covering the period until 2017. These findings are in line with Jordà et al. (2019). Our contribution is to show that they also hold for more recent decades. We also inspect business cycles and find somewhat larger effects on both output and prices during high states of the cycle in line with Tenreyro and Thwaites (2016) and Alpanda et al. (2020).

For the turning point regime classification we find some evidence for stronger effects on GDP during leverage cycle contractions, in line with Harding and Klein (2018). State dependencies weaken once we extend the sample to 2017, which suggests that this effect does not necessarily hold for the euro area after the 2008 Financial Crisis.

We finally explore the larger relative price response in low leverage states. While this can not be explained from a weaker credit channel, we offer an explanation based on higher uncertainty. We first show that a similar pattern also holds for aggregate demand and supply shocks, as identified from sign restrictions in the panel SVAR. We then inspect the in-sample forecast errors from our VAR and find larger forecast errors for prices in low leverage states. Standard price setting models predict that the frequency of price adjustment increases with macroeconomic uncertainty. This is indeed found by studies using firm level data (Vavra, 2014; Bachmann et al., 2019). The resulting increase in aggregate price flexibility would induce a quicker pass-through of monetary policy shocks to inflation and add to a weaker GDP response.

Taken together, the outcomes for level and turning point regimes suggest that monetary policy remains effective at the onset of financial crises (Janssen, Potjagailo, and Wolters, 2019), but is particularly weak during the early stages of recovery from leverage cycle troughs. The level effect is however stronger in our sample. Our findings not only contribute to explaining the need for a prolonged monetary policy stimulus during the recovery from the 2008 Financial Crisis, but also give some insights into inflation dynamics over the most recent leverage cycle, when inflation initially remained low during the Great Moderation and later on declined less then predicted

by linear models (Coibion and Gorodnichenko 2015). Certainly, state dependencies in monetary policy transmission also have profound implications for how monetary models are to be formulated and applied.

The remainder of the paper is organized as follows. Section 2 discusses various properties of leverage cycles and of the corresponding regimes. Section 3 introduces the Threshold Panel SVAR. Sections 4 and 5 present our estimates. Section 6 concludes.

2   Credit and House Price Cycles

Cycles in credit and house prices have been estimated from bandpass filters (Drehmann et al., 2012) and multivariate unobserved components models (Galati et al., 2016; Rünstler and Vlekke, 2018). The two series are found to be subject to medium-term fluctuations with cycle lengths of about 12 to 15 years, clearly beyond business cycle frequencies. The economic significance of the credit cycles is underlined by their predictive power for financial crises and the coincidence of peaks with the onset of financial crises (Schularick and Taylor, 2012; Aikman, Haldane and Nelson, 2015).

In our below panel Threshold SVAR we use regimes based on estimates of credit and house price cycles as predetermined state variables. The requirement of predeterminedness implies a need for one-sided filters. For our baseline estimates we use the one-sided Christiano-Fitzgerald (2003) bandpass filter with a bandwidth of 32 to 80 quarters as proposed by Aikman et al. (2015). We further obtain business cycles from the one-sided filter with a bandwidth of 8 to 32 quarters.

For either filter we employ two different country-specific regime classifications. First, we define high and low states from the level of the estimated cycles. We set state variable $s_{c,t}$ in country c at period t to $s_{c,t} = 1$ if the cycle is positive and $s_{c,t} = 0$ otherwise. Second, we consider a regime classification into expansions and contractions. We apply the turning point analysis of Harding and Pagan (2006) to the cycles to identify peaks and troughs and set $s_{c,t} = 1$ in between a trough and the subsequent peak to mark an expansion and $s_{c,t} = 0$ otherwise. We follow Claessens, Kose and Terrones (2012) with adjusting the turning point algorithm to medium-term cycles.

Our sample covers 10 euro area countries (Austria, Belgium, Germany, Spain, Finland, France, Italy, Ireland, the Netherlands, and Portugal) over the period of 1970 Q1 to 2017 Q4. The data on total credit to the private non-financial sector and residential property prices are taken from the BIS. We deflate both series by the GDP deflator. For house prices we exclude Austria, as the data start only in 1995.[3]

We prefer credit to the non-financial private sector over household credit in order to capture bank net worth effects in a better way. Moreover, a significant share of credit to non-financial corporates takes the form of mortgages, which may create state dependencies from collateral constraints similar to households (Jaccard, 2020). We estimate credit cycles from credit volumes in place of the credit-to-GDP ratio. While the estimates are highly correlated, the former should have better statistical properties, as signal-to-noise ratios are more favorable. Moreover, Repullo and Saurina (2011) show that credit-to-GDP cycles tend to lag changes in financial conditions as output responds faster to shocks than the credit stock. As a result, the credit-to-GDP ratio continues to rise for several quarters after the onset of a financial crisis.[4]

Figure 1 shows the evolution of regimes. The graphs plot the share of countries that face a high state of the cycle or an expansion, respectively, in a given period. For credit and house price cycles synchronization turns out moderate, compared to business cycles. Notwithstanding a high coincidence of regimes at around major peaks and troughs, such as in the early 1990s or after the 2008 Financial Crisis, there are also many episodes with divergent regimes. For credit cycles, for instance, the number of countries facing a high state or an expansion remains within a range of 4 and 7 for more than half of observations. This feature should benefit the estimates of our panel VAR as it reduces cross-sectional dependencies and provides some insurance against common sources of time variation in monetary policy transmission.

The moderate synchronization reflects, to some extent, regional disparities in the evolution of cycles: In 2005, for instance, Germany, the Netherlands, Belgium, and Aus-

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[3]The data are available at https://www.bis.org/statistics.

[4]While this observation motivates Alpanda and Zubairy (2019) to interpret a high state of the credit-to-GDP cycle as an indication of binding collateral constraints, such property arguably applies only to the final stages of the high state. Rünstler and Vlekke (2018) study the properties of one-sided estimates of credit and house price cycles. They conclude that signal-to-noise ratios compare to those of business cycles, as the higher volatility of cycles compensates for the lower frequency.

tria stayed in low credit and house price regimes, while the mediterranean economies faced high states. A reverse though less pronounced pattern had emerged in the mid-1990s and was about to re-appear at the end of our sample, as both credit and house price cycles entered a high state in Germany and several neighboring countries. The synchronization of euro area credit and house price cycles and their regional disparities are studied in more detail by ECB (2018).

Figure 1: Synchronization of Cycles Across Countries
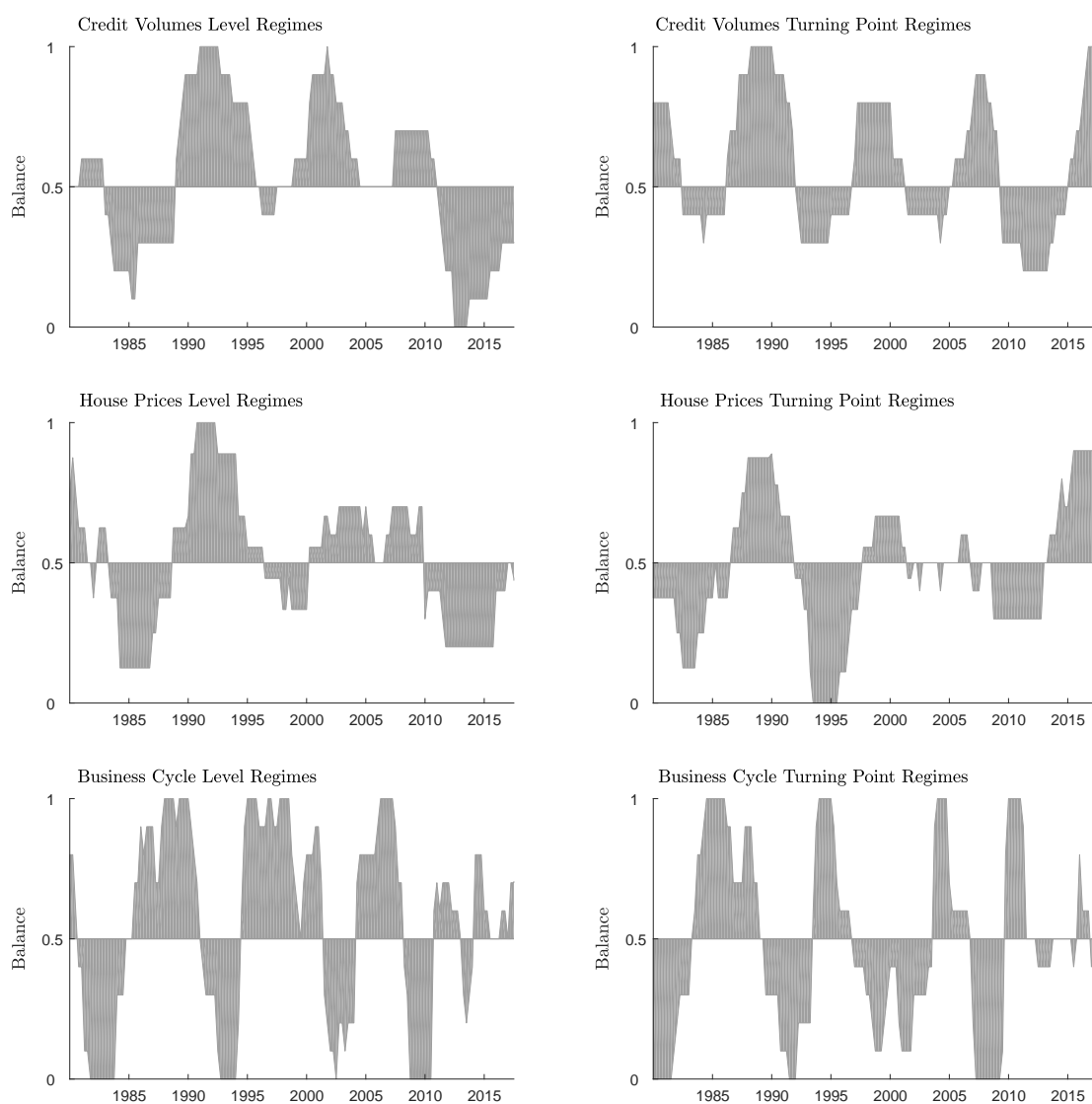
The left-hand graphs show the shares of countries that face a high state of the cycle in a given period, while the right-hand graphs show the shares of countries that face an expansion. The regimes are based on one-sided CF bandpass filters, as described in the main text.

Estimates of cycles and the resulting regimes for the individual countries are shown in Figures Annex A.1, A.4, and A.5, while Table A.2 reports correlations among the cycles. Regime classifications from credit and house price cycles are fairly similar with a median correlation of 0.64 between the two cycles, while correlations with business cycles are very small by construction, as filter bands do not overlap.

3   Methodology

We use a Bayesian Structural Threshold Panel VAR for GDP, CPI inflation, and the policy rate. For each country, we split the observations into two samples according to state variable $s_{c,t}$, which defines the country-specific cyclical regimes. We then estimate a Bayesian panel VAR separately for the two regimes using a partial shrinkage prior to impose similar dynamics across countries. For each regime, we finally identify monetary policy shocks from a combination of sign and magnitude restrictions. Our approach to imposing shrinkage in estimation and identification steps is fairly standard to the literature based on Canova (2005) and Canova and Pappa (2007).

The use of a panel VAR with Bayesian shrinkage should substantially enhance the robustness of our findings compared to estimates based on individual country estimates. At the same time, partial shrinkage estimators have been found to be fairly robust against cross-sectional dependencies (Hsiao, Pesaran, and Tahmiscioglu 1999).

The reduced-form threshold panel VAR is given by

$$x_{c,t} = \Gamma_c(s_{c,t-h})z_{c,t} + \sum_{p=1}^{P} B_{cp}(s_{c,t-h})x_{c,t-p} + u_{c,t}, \qquad u_{c,t} \sim N(0, \Sigma_c(s_{c,t-h})), \quad (1)$$

with countries $c = 1, \ldots, C$ and observations $t = 1, \ldots, T$. The $n \times 1$ vector $x_{c,t}$ represents the endogenous variables of the VAR, while $z_{c,t}$ is an $m \times 1$ vector of predetermined variables including deterministic components. VAR coefficients $B_{cp}(s)$ and $\Gamma_c(s)$ depend on predetermined state variables $s_{c,t-h}$, $h > 0$, which may take a value of zero or one. Residuals $u_{c,t}$ are assumed to be normally distributed with state-dependent covariances, $u_{c,t} \sim N(0, \Sigma_c(s))$, and are independent over time.

We follow Jarocinski (2012) in imposing partial shrinkage on the parameters of the

reduced-form VAR. Denote with $B_{c,+}(s) = (B_{c1}(s), \ldots, B_{cP}(s))$ the matrix of stacked autoregressive coefficients for country c and let $\beta_{cs} = \text{vec } B_{c,+}(s)$. The partial shrinkage model assumes that country coefficients β_{cs} are drawn from a normal distribution with common mean β_s and covariance matrix $\lambda\Omega$,

$$p(\beta_{cs}|\beta_s, \lambda) = N(\beta_s, \lambda\Omega). \tag{2}$$

Parameter λ determines the overall degree of shrinkage in coefficients β_{cs} and Ω is a pre-specified diagonal matrix adjusting for parameter-specific tightness. Parameter λ is subject to an inverse-gamma prior, $p(\lambda) \propto \lambda^{1-v/2} \exp(-\frac{s}{2\lambda})$. The diagonal element Ω_{kk} related to coefficient $B_{cp,ij}$ is specified as $\Omega_{kk} = \widehat{\sigma}_i^2/\widehat{\sigma}_j^2$, where $\widehat{\sigma}_i^2$ is the sample residual variance of a pooled univariate 4th-order autoregression of series y_{ti}.

Model (1) is completed by assuming uninformative priors for pooled coefficients $p(\beta_s) \propto 1$, and residual covariances $p(\Sigma_c(s)) \propto |\Sigma_c(s)|^{-\frac{1}{2}(n+1)}$. For coefficients $\Gamma_c(s)$ we use uninformative priors as well. For each country, we split the observations in two samples according to the values of $s_{c,t-h}$ and estimate the parameters separately for each regime under the shrinkage prior. We obtain draws from the posteriors of $B_{c,+}(s)$ and $\Gamma_c(s)$ from the Gibbs sampler of Jarocinski (2012).

Given posterior draws of parameters $B_{c,+}(s)$, we then identify monetary policy (and possibly other) shocks from sign and magnitude restrictions on impulse responses (IRFs). Generally, the purpose is to identify the set of matrices $A_c(s)$ that defines structural shocks $\varepsilon_{c,t} = A_c(s)u_{c,t}$ such that shocks are distributed as $\varepsilon_{c,t} \sim N(0, I_n)$ and restrictions on IRFs are satisfied. This results in the SVAR representation

$$x_t = \Gamma_c(s_{c,t-h})z_{c,t} + \sum_{p=1}^{P} B_{cp}(s_{c,t-h})x_{c,t-p} + A_c^{-1}(s_{c,t-h})\varepsilon_{c,t}, \qquad \varepsilon_{c,t} \sim N(0, I_n). \tag{3}$$

For an individual country c and a given state s, the set of matrices $A_c(s)$ that satisfy the condition $\varepsilon_{c,t} \sim N(0, I_n)$ can be represented as $A_c(s) = A_c^*(s)Q_c(s)$, where $A_c^*(s)$ is the Choleski decomposition of residual covariances $\Sigma_c^{-1}(s) = A_c^*(s)A_c^*(s)^T$ and $Q_c(s)$ is an arbitrary orthogonal matrix. Given $B_{c,+}(s)$ and $A_c^*(s)$, the identifying restrictions define a set of admissible rotations $Q_c(s)$. Arias, Rubio-Ramirez, and

Waggoner (2018) propose to generate posterior draws of matrix $Q_c(s)$ from rejection sampling, by obtaining uninformative draws of $Q_c(s)$ based on the Haar probability measure and accepting those draws that fulfill the identifying restrictions.

Following Canova and Pappa (2007) and Gambacorta et al. (2014), we achieve shrinkage across countries in the identification step by imposing a pooled matrix $Q_c(s) = Q(s)$. We then consider the median response across countries. We therefore proceed as follows in generating draws of IRFs under regime s: (i) we draw from the posteriors of $B_{c,+}(s)$ and obtain Choleski decompositions $A_c^*(s)$ of the resulting estimates of residual covariances; (ii) we draw a single orthogonal matrix $Q(s)$ from the Haar probability measure and generate matrices $A_c^{-1}(s) = A_c^*(s)Q(s)$; (iii) we obtain the resulting IRFs for each country and accept the draw of $\{B_{c,+}(s), Q(s)\}$ if the median country IRF satisfies the identifying restrictions.

4 State-Dependent Impulse Responses

Our VAR includes the log-level of GDP ($y_{c,t}$), the quarterly change in the log of the CPI ($\Delta p_{c,t}$), and a policy rate ($r_{c,t}$). We measure the policy rate by national 3-month short term rates before 2000 Q1, the euro area 3-month money market rate in between 2000 Q1 and 2007 Q4, and the euro area shadow rate of Krippner (2013) thereafter. We use three lags of the series and remove a linear trend from GDP before including it into the VAR. We further add the U.S. 3-month money market rate (replacing it by the Krippner (2013) shadow rate after 2007 Q4) and the log-differences of world commodity prices and U.S. GDP as predetermined variables at lag 1 to the VAR. We set $s = v = 0$, resulting in a uninformative prior for λ.[5]

Our estimation sample covers the 10 euro area countries listed in section 2 over the period of 1982 Q1 to 2017 Q4. However, in line with Tenreyro and Thwaites (2016) and Alpanda and Zubairy (2019) we restrict the sample to end in 2007 Q4 for our main estimates in order to avoid that the results are driven solely by the 2008 Financial

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[5]We use the predetermined series in first differences as we are primarily interested in accounting for their short-term effects on the euro area. Shadow rates are downloaded from the website of the Reserve Bank of New Zealand. All other data are taken from the ECB Statistical Data Warehouse.

Crisis. Moreover, while estimates of the shadow rate aim at accounting for the zero lower bound and non-standard policies after 2008, they have been found to be rather sensitive to model specification (Christensen and Rudebusch, 2013; Lombardi and Zhu, 2014), which might impair the identification of policy shocks.

4.1 Identifying Restrictions

We identify monetary policy shocks from a combination of sign and magnitude restrictions on impulse response functions (IRFs). Sign restrictions have been widely used for this purpose (Uhlig, 2017). More recently, Wolf (2017) and Volpicella (2019) have proposed to complement sign with magnitude restrictions in order to limit the size of the monetary multiplier on impact and thereby tighten up identification.[6]

We impose the restrictions that a positive policy shock increases the short-term rate on impact and induces declines in output and inflation at horizons of two to four quarters. In addition, we restrict the contributions of monetary policy shocks to the one-step ahead forecast error variances of output and inflation to remain below a certain threshold. For our baseline estimates we impose a moderate threshold of 33%, which implies that the contributions of monetary policy shocks on impact remain below the average contributions of the remaining shocks in the system. Our identification scheme reflects a minimal set of conventional beliefs about the effects of monetary policy shocks and ensures that restrictions hold in both regimes.[7]

The magnitude restrictions act to limit the absolute size of output and price responses on impact. They may be regarded as a soft version of the respective zero restrictions that have been proposed by Christiano, Eichenbaum and Evans (1999) and used in a large number of studies (Ramey, 2016). The combination of sign and magnitude restrictions has however the advantage that it avoids the so-called price puzzle, i.e. a counter-intuitive sign of the initial inflation response, which is pervasive to many studies. Uhlig (2017) points to the logical inconsistencies that arise from the assumption that output and prices respond on impact to all types of news apart from those

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[6]For further applications see Kilian and Murphy (2012) and De Santis and Zimic (2018).

[7]Studies on state dependencies often find counter-intuitive responses in one of the states, which arguably weakens their conclusions (see Tenreyro and Thwaites, 2016).

on monetary policy. With magnitude restrictions, such assumption is avoided while the notion is maintained that the monetary multiplier on impact remains small.

Figure 2 shows that the magnitude restrictions reduce the scale of output and price responses to a considerable extent and put them more in line with estimates based on zero restrictions.[8] They also narrow the credible set of IRF estimates. This appears to stem mostly from the elimination of draws that result in small and very short-lived responses of the short-term rate, relative to GDP and inflation. Wolf (2017) relates the emergence of such draws to the blurring effects of linear combinations of other shocks in the system. Results from further specifications are shown in Figure Annex A.11. A tighter magnitude threshold of 20% has little impact on median estimates of IRFs, but further narrows down credible sets. Even lower thresholds would re-enact the price puzzle and thereby be incompatible with our sign restrictions.
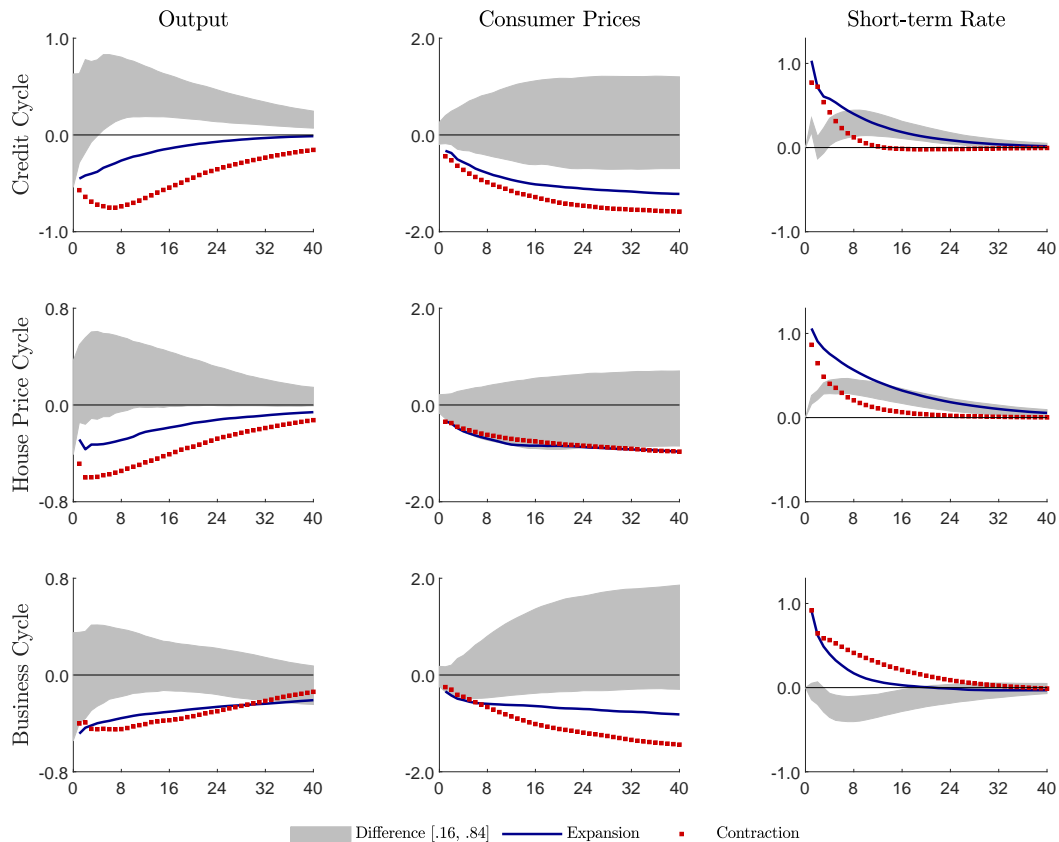
Figure 2: Impulse Responses from Linear VAR
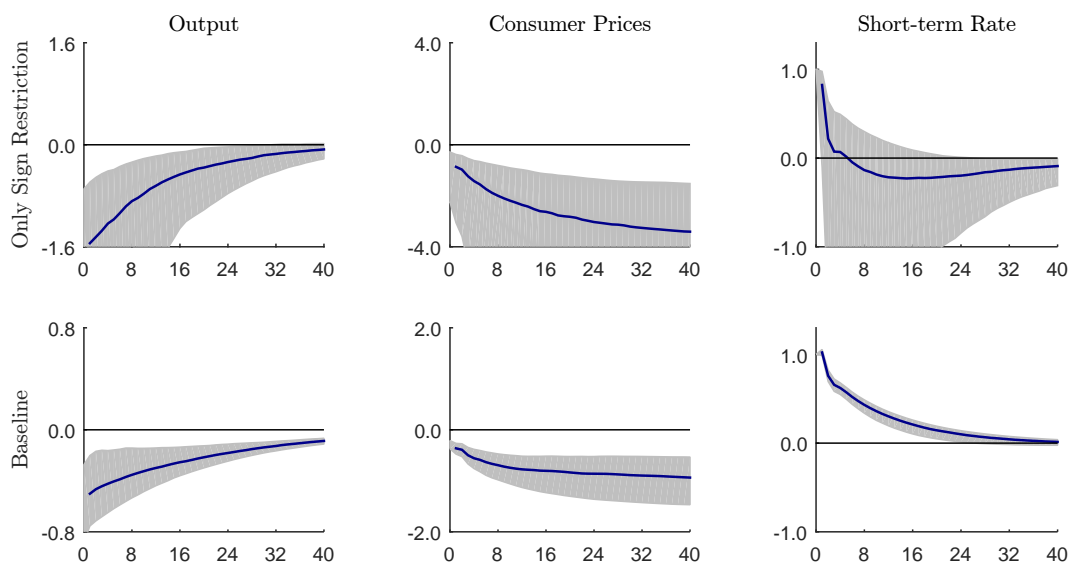
The plots show the median IRFs to a monetary policy shock scaled to a 100 basis points increase in the short-term rate. The shaded areas show 0.16 and 0.84 quantiles. The upper row shows results for sign restrictions, the lower row adds magnitude restrictions with a threshold of 33%. The estimation sample ranges from 1982 Q1 to 2007 Q4.

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[8]For the euro area, Peersman and Smets (2002) estimate a maximum response of output of about 0.35% to a monetary policy shock of 100 basis points, while Jarocinski (2012) finds a value of 0.5% for industrial production. See Ramey (2016) for a comparison of identification schemes for the U.S.

4.2 State-Dependent Impulse Responses

The upper two panels of Figure 3 plot the impulse responses to a 100 basis points contractionary policy shock under high and low states of credit and house price cycles as obtained from the CF filter. For credit and house price regimes we use a lag of $h = 5$ for state variables $s_{c,t-h}$ in the VAR, as this provides sharper results than a value of $h = 1$. This also allows us to apply a partly two-sided CF filter that uses observations up to four quarters ahead and thereby is subject to lower uncertainty. For business cycle regimes we use a lag of $h = 1$.

Figure 3: State-Dependent Impulse Responses for Level Regimes
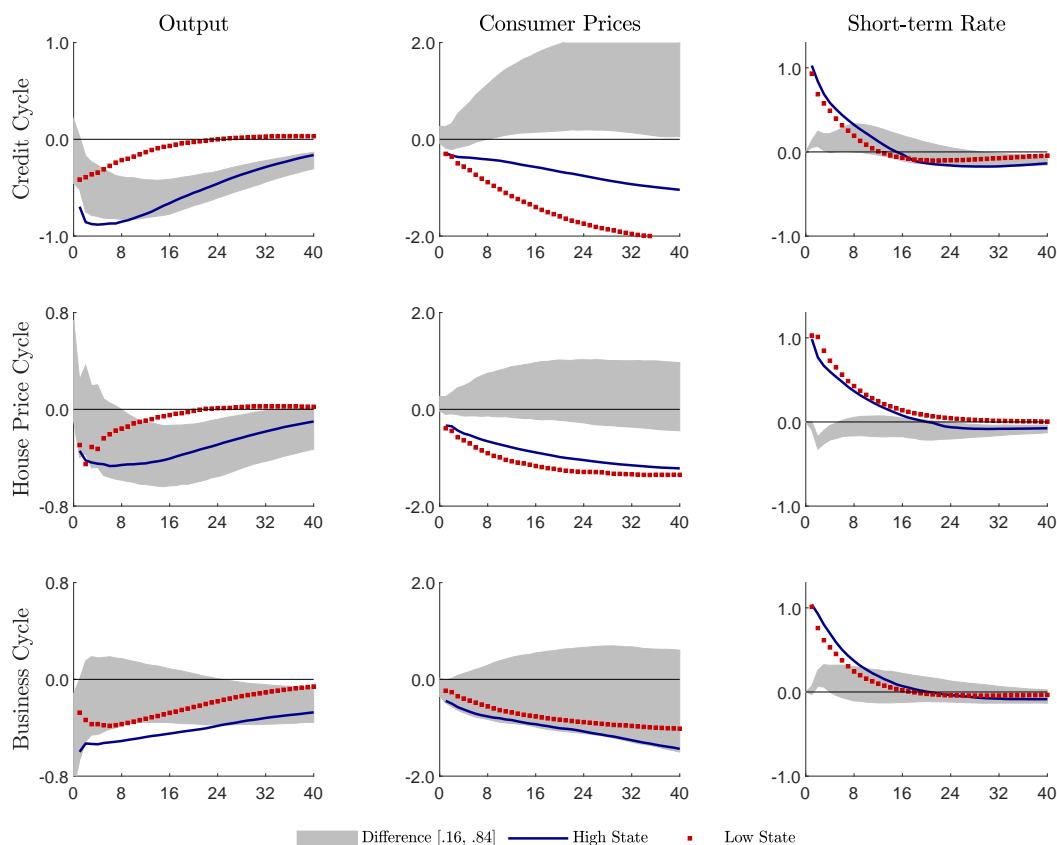
The plots show the median IRFs to a monetary policy shock scaled to a 100 basis points increase in the short-term rate. Blue solid (red) dotted lines show the effects in high (low) states of the cycles. The shaded areas show credible sets for the differences between regimes based on 0.16 and 0.84 quantiles. The sample ranges from 1982 Q1 to 2007 Q4.

We find that the responses of both output and consumer prices differ across the two regimes in opposite ways. The response of GDP is smaller and less persistent in low states of the cycles compared to high states. The differences remain moderate on impact. However, during low states of credit and house price cycles the response lacks the usual hump-shaped pattern and output returns to baseline relatively quickly. For high states, we find a more persistent hump-shaped response with a peak at 4 to 6 quarters. By contrast, the response of prices is larger in high than in low states of the cycle. While the difference is small for house price regimes, taken together the results imply a marked shift in the inflation-output trade-off across regimes. Note also that differences in the paths of the policy rate remain small.

Figure 4: State-Dependent Impulse Responses for Turning Point Regimes

The plots show the median IRFs to a monetary policy shock scaled to a 100 basis points increase in the short-term rate. Blue solid (red dotted) lines show the effects in expansion (contraction) regimes. See Figure 3 for further explanations.

The bottom panel of Figure 3 presents estimates for business cycle level regimes from the CF business cycle filter. We again find some, though milder, evidence for a larger response of output to monetary policy shocks during high states of the cycle. Contrary to leverage cycles, however, state dependencies in prices go in the same direction and there is no shift in the inflation-output trade-off.

The results for expansion and contraction regimes based on turning points in leverage and business cycles are shown in Figure 4. For both credit and house price cycles we find a larger response of output to monetary policy shocks during leverage cycle contractions, but again the effects are more pronounced for credit cycles. For the latter, we also find weak state dependencies in prices. Again, these go in the same direction as output and the output-inflation trade-off therefore remains fairly stable across regimes. For business cycles, we find weak state dependencies in prices only.

Taken together, the outcomes for level and turning point regimes suggest that monetary policy transmission is particularly weak at the early stages of recovery from leverage cycle troughs, when the cycle is still in a low state, but remains effective at the onset of financial crises. The latter effect has been found by Janssen et al. (2019) from a narrative approach using the database of Laeven and Valencia (2018).

4.3 Robustness Checks

In this section, we summarize the findings from robustness checks related to alternative regime definitions, the extension of the sample to 2017 Q4, and alternative VAR specifications. For level regimes our baseline results appear robust, with sharper outcomes for house price regimes in many cases. However, state dependencies in turning point regimes weaken for the alternative filters and for full-sample estimates. The results from the alternative estimates are shown in Figures Annex A.12 to A.28.

Alternative regime classifications. We inspect the robustness of our findings with respect to regime classifications from two alternative filters. First, we use the regression filter of Hamilton (2018) filter for business cycles and its adaptation to medium-term credit cycles by Drehmann and Yetman (2018). For the latter, we regress each series

on its own lags at 20 to 23 quarters and then smoothen the residual from a four-quarter moving average. Second, we use the three-year growth rate in credit and house prices, as used by Jordà et al. (2019). We set the state to high if the growth rate is above its country mean. We also report results for the credit-to-GDP ratio.

Figures Annex A.1 to A.10 present the resulting cycles and regime classifications, while Tables A.1 to A.3 show corresponding correlations. The various filters give similar outcomes with country medians of correlations among cycles above 0.6. Cycles based on three-year growth rates yet tend to lead those based on the bandpass filter by about six quarters. Credit-to-GDP cycles are highly correlated with those in credit volumes, but tend to lag cycles in the other series by one to three quarters.

For level regimes we obtain robust results. All filters give pronounced state dependencies for cycles in credit volumes and the credit-GDP ratio, with the exception of credit-to-GDP cycles from the regression filter. For house price cycles, the alternative filters actually give sharper results than our baseline estimates. While the alternative filters generally find weak state dependencies in prices, the shift in the inflation-output trade-off across regimes remains.

Estimates for credit and house price turning point regimes from the regression filter do not yield any state dependencies. This may stem from the fact that the filter, in contrast to the CF filter, does not eliminate business cycle frequencies, which results in additional fluctuations affecting turning point analysis. For business cycles we find larger output responses both during high states of the cycle and during expansions.[9]

Full-sample estimates. Our results for credit and house price level regimes carry over to the estimates based on the full sample until 2017 Q4. However, state dependencies with respect to turning point regimes now turn out weak for the bandpass filter as well. This may reflect the observation made by many observers (e.g. Hartmann and Smets, 2018) that monetary policy transmission in the euro area was impaired during the contraction after the 2008 Financial Crisis. For business cycles, the full-sample estimates do not find any state dependencies.

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[9]Correlations between medium-term and business cycles from the CF filter are very small by construction, as filter bands do not overlap. Table A.3 shows small negative correlations, while those obtained from the regression filters are significantly positive.

Alternative specifications. Our results also remain fairly robust for VAR specifications that include the underlying state variable as an endogenous variable. That is, for models based on credit and house price regimes, we add credit and house prices, respectively, in log-levels to the VAR. We impose the same magnitude restrictions as for GDP and inflation on the additional series. The results for credit cycles are shown in Figure 5. They yield somewhat larger output and price responses than the baseline estimates. State dependencies in credit are in line with those in output.[10]

Finally, our findings are robust to using sign restrictions at alternative horizons, a tighter magnitude threshold of 20%, the state at lag $h = 1$ in equation (1) instead of $h = 5$, or three lags of the predetermined variables. In many cases, we find sharper state dependencies in prices for level regimes compared to our baseline estimates.

Figure 5: State-Dependent Impulse Responses for VAR with Credit
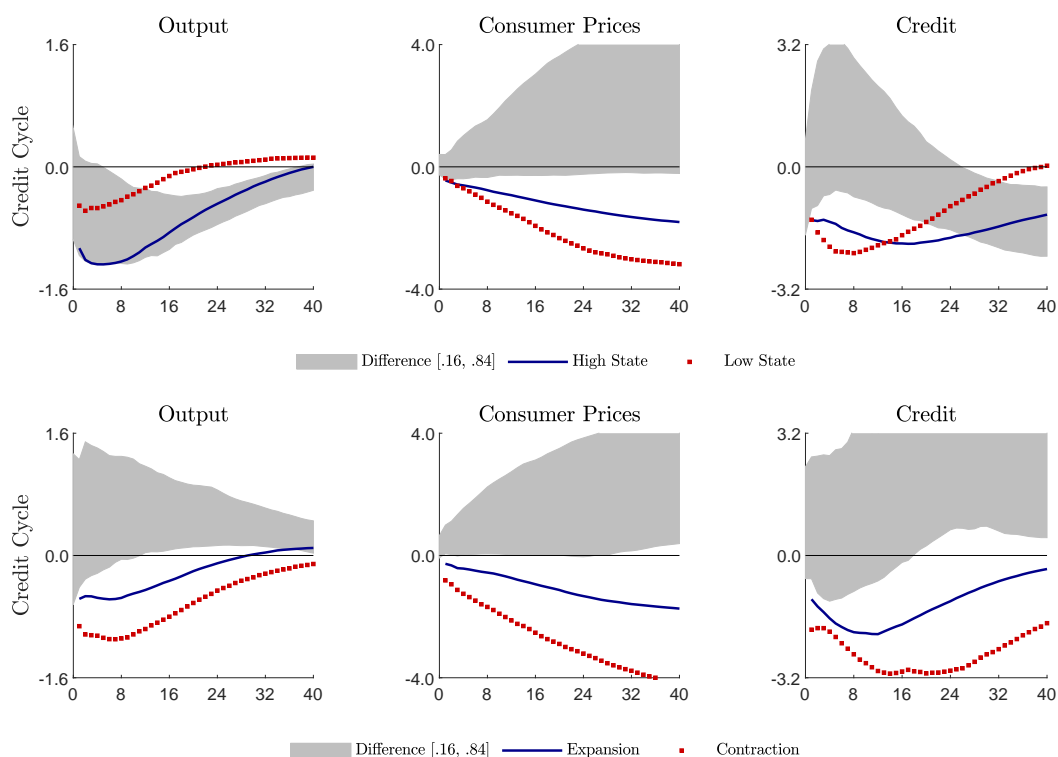
The plots show the median IRFs to a monetary policy shock scaled to a 100 basis points increase in the short-term rate. The VAR is extended with credit volumes. Regimes are based on credit cycle from the CF filter. See Figures 3 and 4 for further explanations.

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[10]We include only the underlying state, as our samples differ for credit and house prices.

5 Leverage Cycles and Uncertainty

A weaker credit channel of monetary policy does not explain the larger impact of policy shocks on prices during low states of the leverage cycle. One possible explanation for this finding is higher aggregate price flexibility during low states. de Groot (2014) and Eickmeier, Metiu, and Prieto (2016) point to a negative relationship between uncertainty and leverage as low uncertainty creates incentives for risk-taking.

As pointed out by Vavra (2014) and Baley and Blanco (2019), standard price-setting models predict that higher macro-economic uncertainty generates more frequent price adjustments. The resulting higher aggregate price flexibility gives rise to a quicker pass-through of monetary policy to inflation and correspondingly curtails the GDP response. A more frequent price adjustment of firms in periods of high uncertainty has been documented by various studies based on micro data (Dixon et al., 2014; Bachmann et al., 2019). Consequently, higher uncertainty may contribute to explaining the state dependence of the inflation-output trade-off in our estimates.

In the remainder of this section we provide two pieces of evidence for this hypothesis. We first show that the larger inflation-output trade-off in low states is a general phenomenon and also applies to other types of shocks. We then run an in-sample forecast exercise and find larger forecast errors for inflation in low states.

5.1 Are State Dependencies Confined to Monetary Policy?

With our VAR with three series, we approach the first issue from a decomposition of the VAR residuals into monetary policy, aggregate demand (AD), and aggregate supply (AS) shocks. We identify the alternative shocks from sign restrictions. Specifically, we impose the restrictions that a positive AD shock triggers an increase in GDP on impact together with increases in inflation and the policy rate after two quarters. A positive AS shock is restricted to raise GDP and to reduce inflation at a horizon of two quarters. Similar restrictions have, for instance, been used by Buch, Eickmeier, and Prieto (2014). Restrictions on monetary policy shocks are as above, but with sign restrictions imposed only at a horizon of two quarters for the sake of symmetry.

The results for credit cycle level regimes are shown in Figure 6. They suggest that the larger inflation-output trade-off in low leverage states is a general phenomenon that applies also to other types of shocks. In the absence of a well-defined anchor for scaling the IRFs to the alternative shocks, we show them for shocks of size unity, which implies that the differences between regimes should be interpreted only in relative terms. For both aggregate demand and supply shocks we find little difference in the responses in GDP and short-term rates across regimes, but larger responses of prices in low states of the cycles. Hence, shifts in the output-inflation trade-off across regimes are similar to those for monetary policy shocks. Given the similar pattern for both shocks, this conclusion is independent of the details of our identification scheme.

Figure 6: State-Dependent Impulse Responses: AD and AS Shocks
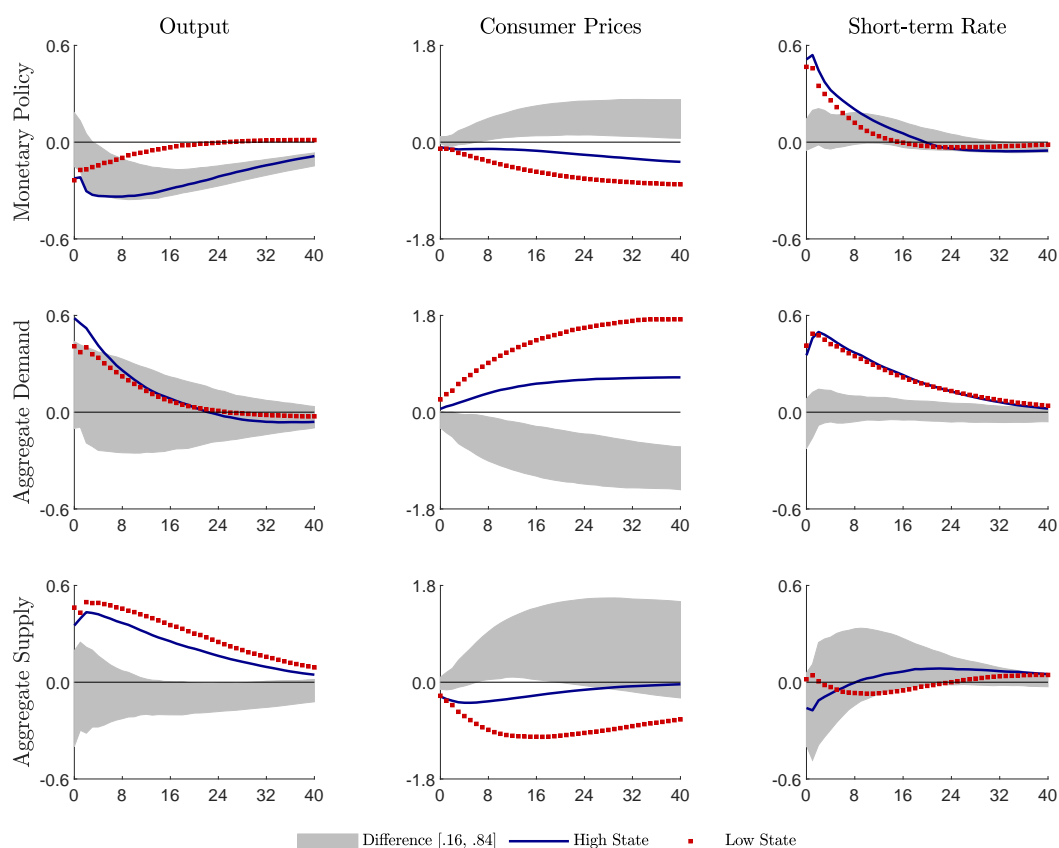
The plots show the median IRFs to a monetary policy shock scaled to a 100 basis points increase in the short-term rate. Regimes are based on cycles in credit volumes from the CF filter. See Figure 3 for further explanations.

5.2 Forecast errors

We assess shifts in uncertainty across level and turning point regimes from the forecast errors of our Threshold Panel VAR. The upper panel of Table 1 shows the RMSE of forecast errors in the three series across level regimes, based on credit cycle from the CF filter. The lower panel shows the corresponding results for turning point regimes. The table reports the non-weighted country average of forecast errors. It also includes the results from the random walk forecast and the linear panel VAR.

We find higher forecast uncertainty during low states of the credit cycle compared to high states for all three series. For GDP these differences accrue mostly for one-quarter ahead forecasts and remain moderate. For prices and the short-term rate differences are sizable at horizons of one and two years. Differences across turning point regimes remain small, with the exception of higher forecast uncertainty in the short-term rate during expansions. Finally, Table 1 also documents gains in forecast accuracy from the Threshold VAR against the linear VAR.

Table 1: In-Sample Forecasting Performance Across Credit Regimes

		Random Walk			Linear VAR			Threshold VAR		
Level Regimes		y_t	p_t	r_t	y_t	p_t	r_t	y_t	p_t	r_t
High	1	.87	.54	.85	.84	.31	.79	.81	.30	.79
	4	1.95	1.89	1.88	1.70	.90	1.45	1.59	.79	1.41
	8	3.26	3.63	2.62	2.54	1.77	1.63	2.45	1.44	1.47
Low	1	1.02	.73	.86	1.00	.42	.82	.96	.41	.78
	4	1.80	2.67	1.93	1.69	1.39	1.66	1.62	1.32	1.51
	8	2.96	5.24	2.84	2.55	2.64	2.19	2.43	2.45	2.01
Turning Point Regimes		y_t	p_t	r_t	y_t	p_t	r_t	y_t	p_t	r_t
Expansion	1	.98	.62	.98	.95	.37	.90	.94	.37	.90
	4	2.05	2.21	2.13	1.81	1.17	1.83	1.79	1.14	1.77
	8	3.36	4.34	2.77	2.75	2.13	2.17	2.26	2.02	2.06
Contraction	1	.92	.60	.66	.91	.33	.65	.86	.31	.62
	4	1.75	2.19	1.59	1.68	1.08	1.15	1.51	.99	1.11
	8	2.99	4.24	2.70	2.49	2.32	1.58	2.21	2.01	1.42

The table shows the root mean squared error of 1,4 and 8 step-ahead in-sample forecasts for GDP (y_t), consumer prices (p_t), and the short-term rate (r_t) conducted over the period of 1983 Q1 to 2015 Q4. Credit level and turning point regimes are based on the CF filter.

6 Conclusions

For our sample of 10 euro area economies over the last four decades we find state dependence in monetary policy transmission for both the level and the momentum of the leverage cycle. Monetary policy shocks appear to have had larger effects on output, but a muted impact on inflation, during high states of the cycle and larger effects on output and inflation during contractions. Altogether the results suggest that monetary policy was effective at the onset of financial crises, but particularly weak during the early stages of recovery from leverage cycle troughs. However, the level effect arises as the stronger and more robust one in our estimates. For business cycles we found some evidence for larger effects during high states of the cycle.

Our findings help to explain the need for prolonged monetary policy stimuli during the recovery from financial crises. They also provide some insights into inflation dynamics over the last cycle, when inflation remained low during the 'Great Moderation' despite loose monetary policies, and declined less than predicted by linear models during the 'Great Recession' (Coibion and Gorodnichenko 2015; Bobeica and Jarocinski, 2019).

At the same time, the different outcomes for level and momentum effects suggest that results on state dependencies of monetary policy are sensitive to regime definitions. This may partly explain the different outcomes of related studies.[11] Our results on level effects are closest to Jordà et al. (2019), which are based on annual historical data, while our results on momentum effects are similar to Harding and Klein (2018).

Finally, the findings have various potential implications for the conduct of monetary policy. For instance, the low effectiveness of monetary policy in restoring output during low states of the leverage cycle strengthens the case for 'leaning against the wind' strategies to curb the latter (Svensson, 2017; Gourio, Kashyap and Sim, 2018). Our results also may have implications for macro-economic imbalances in a monetary union, as monetary policy shocks may induce divergence in economic activity and inflation between countries at different stages of the leverage cycle.

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[11]Similar considerations apply to studies on the business cycle. Studies reporting stronger effects of monetary policy in recessions (Peersman and Smets, 2002; Lo and Piger, 2005) typically define regimes from GDP growth, while recent studies reporting stronger effects in booms use a level concept (Jordà et al., 2019; Alpanda et al., 2020).

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Figure A.1: Bandpass (CF) Filter Level Regimes
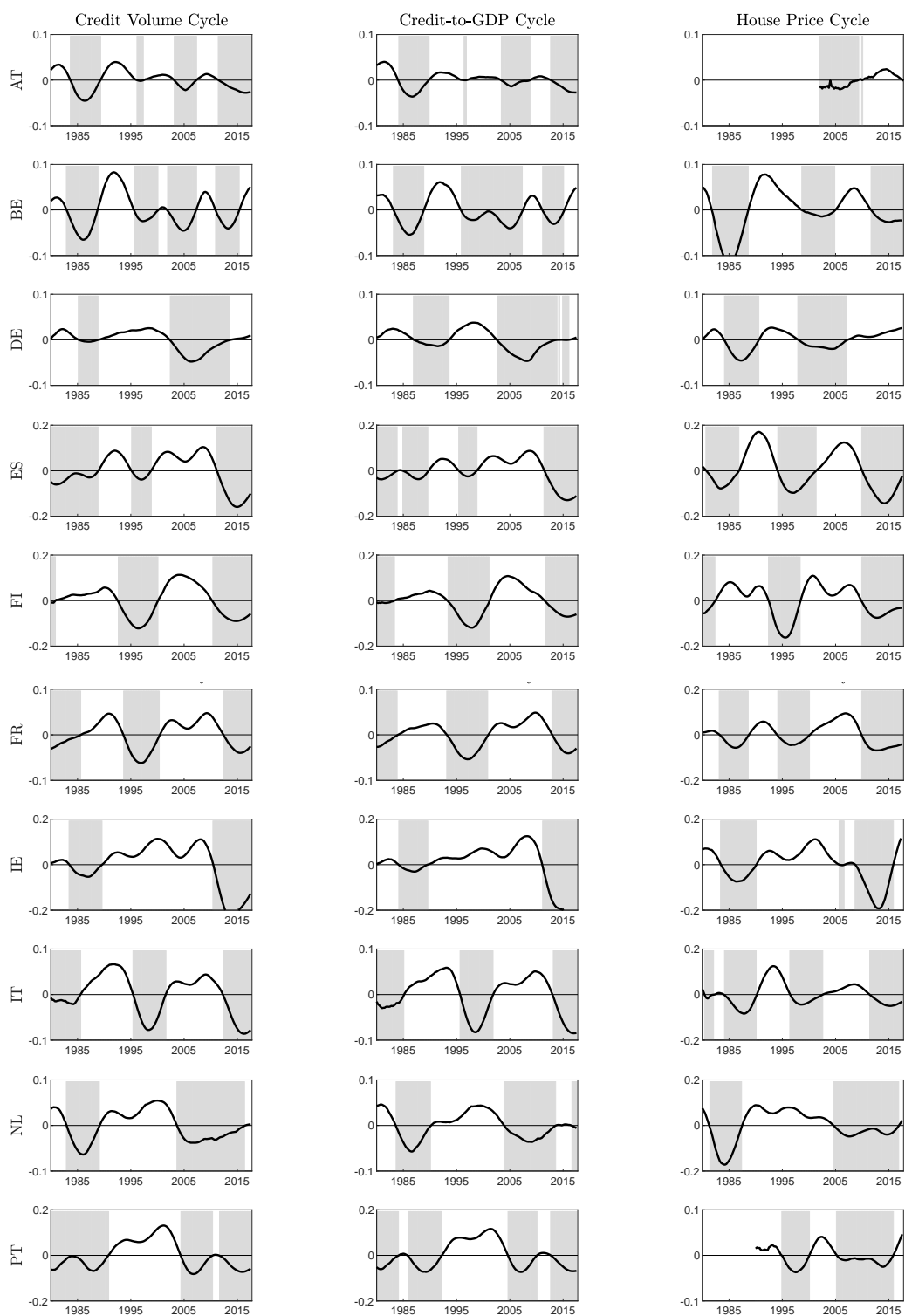
Estimates are based on the CF filter with a filter band of 32-80 quarters. The shaded areas show the low states.

Figure A.2: Regression Filter Level Regimes
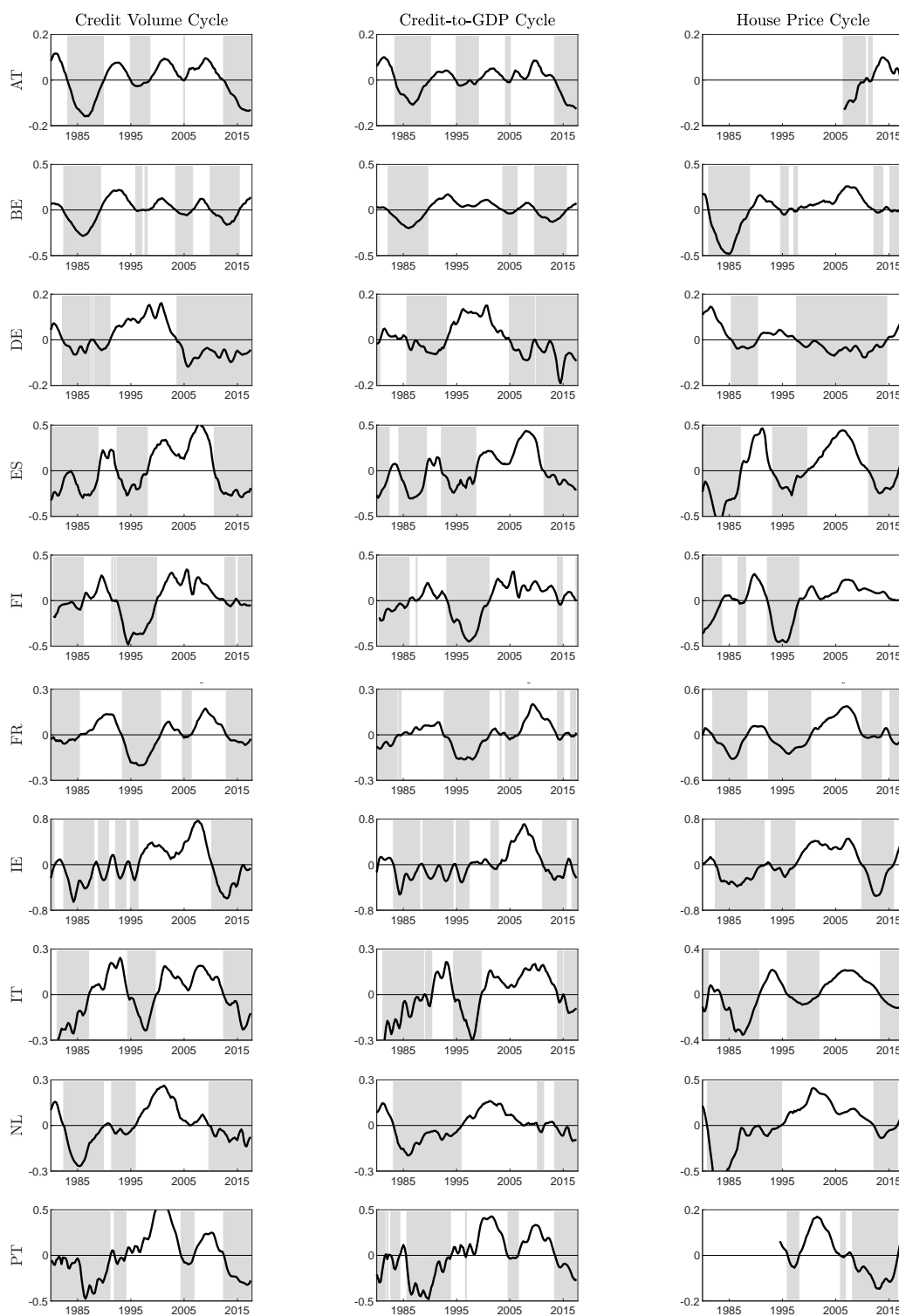
Estimates of cycles are based on the filter by Drehmann and Yetman (2018). The shaded areas show the low states.

Figure A.3: Three-Year Growth Rate Regimes
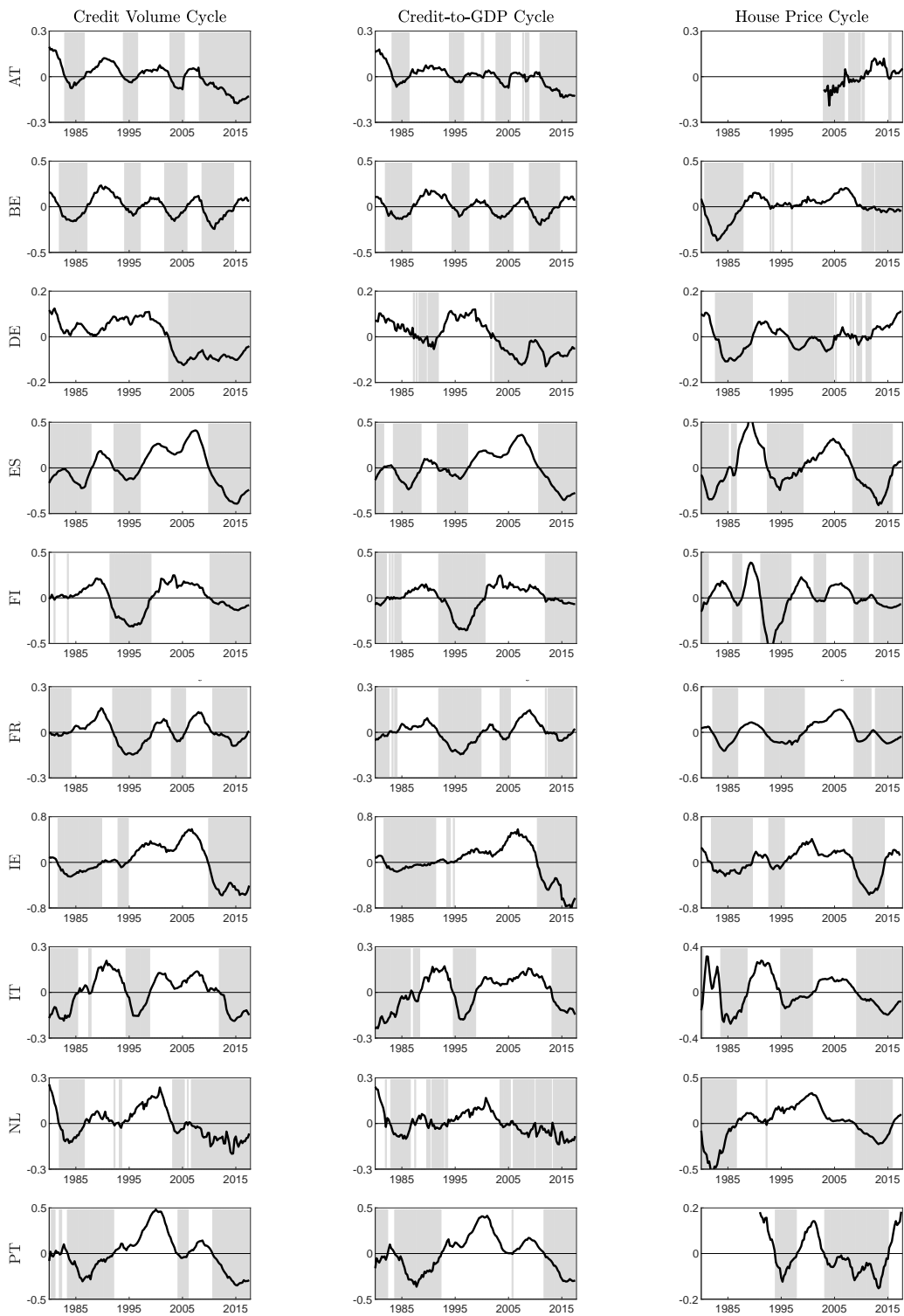
Estimates of cycles are based on the three-year growth rate of series. The shaded areas show the low states, defined as the growth rate being below its country average

Figure A.4: Business Cycle Level Regimes
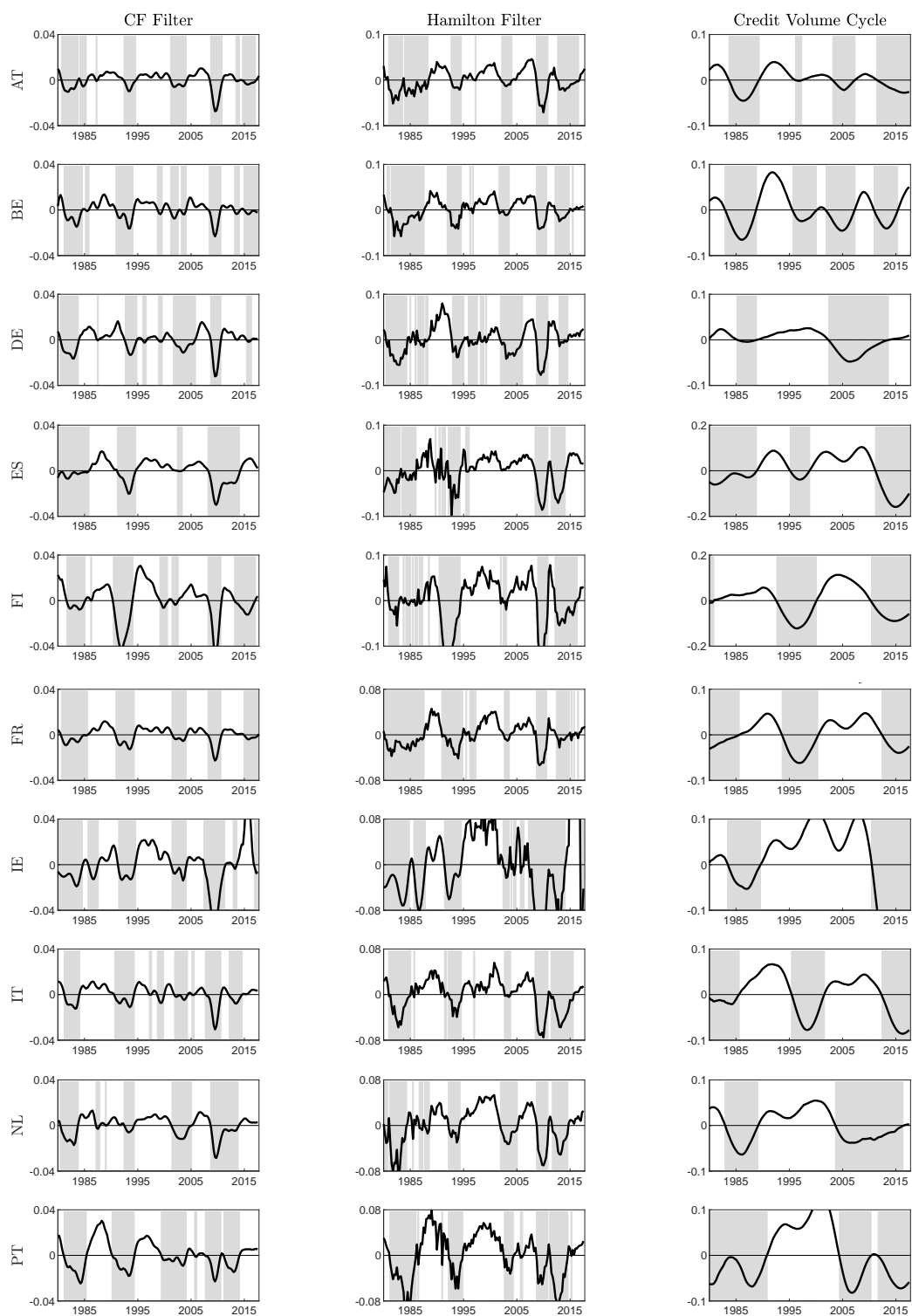
Estimates are based on the CF filter with a filter bands of 8-32 quarters and the Hamilton (2018) filter. For comparison purposes, the right column shows estimates of the credit cycle based on the CF filter (see Figure A.1). The shaded areas show the low states.

Figure A.5: Bandpass (CF) Filter Turning Point Regimes
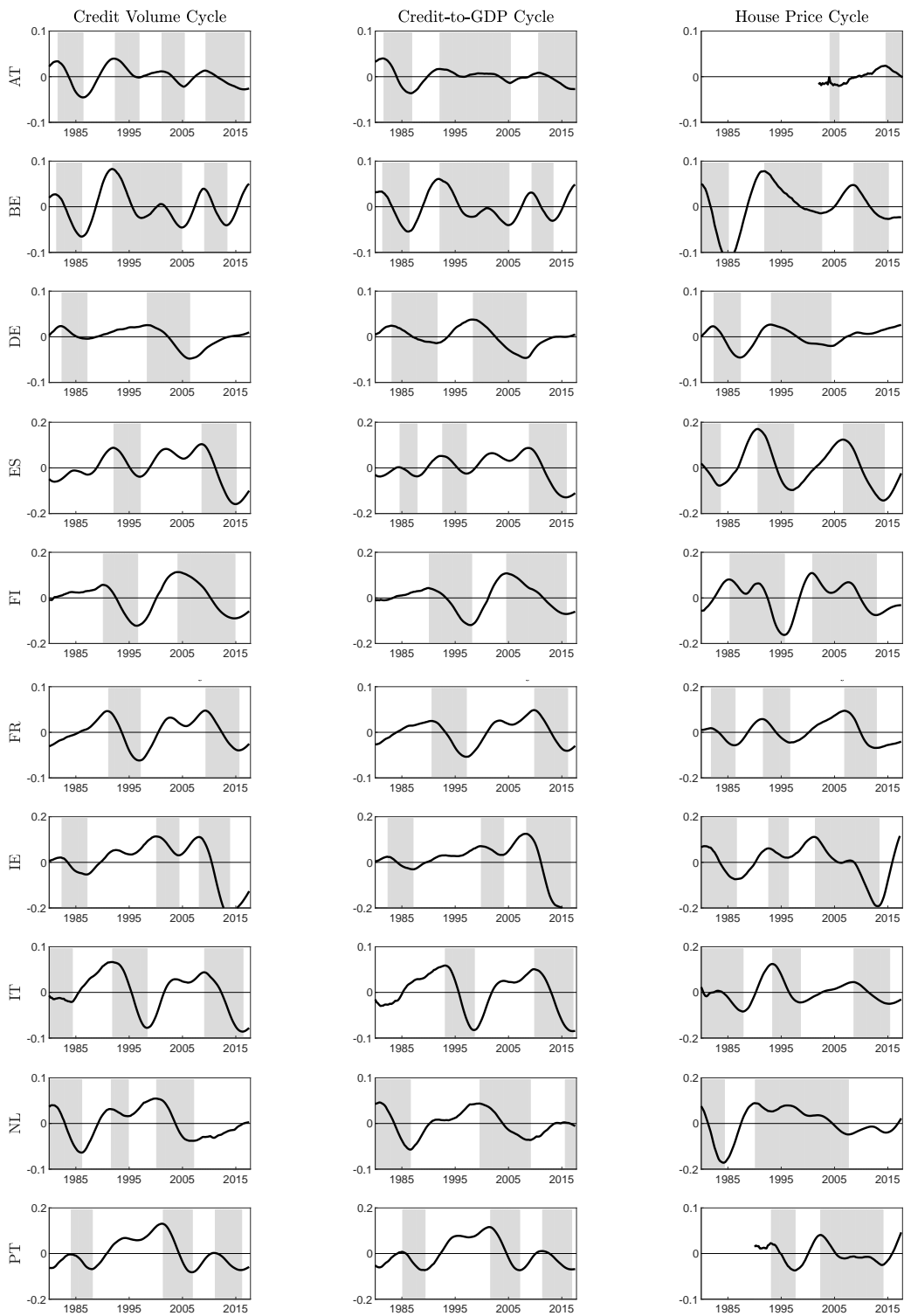
Estimates are based on the CF filter with a filter band of 32-80 quarters. The shaded areas show the contraction regimes based on turning point analysis.

Figure A.6: Regression Filter Turning Point Regimes
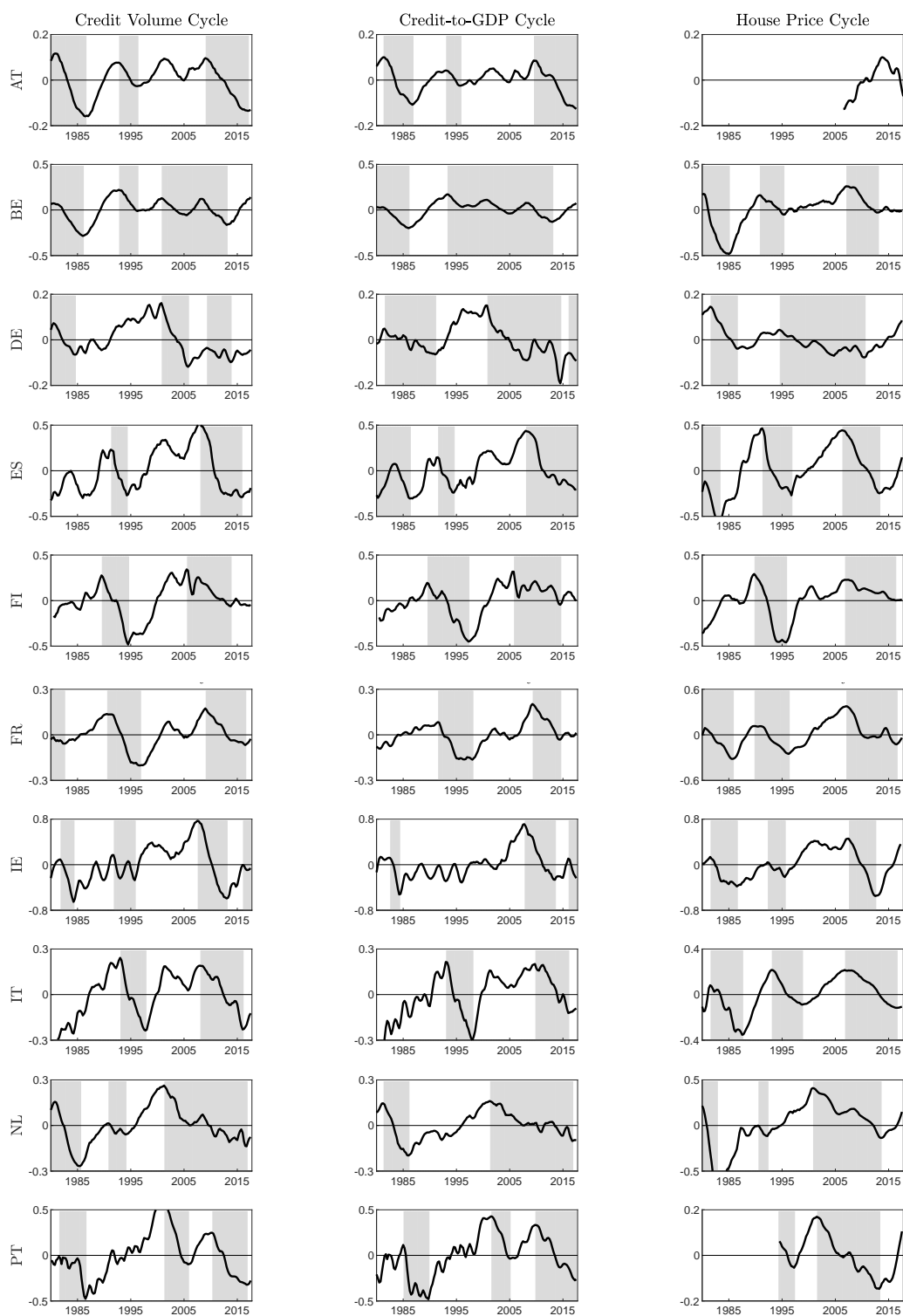
Estimates of cycles are based on the filter by Drehmann and Yetman (2018). The shaded areas show the contraction regimes based on turning point analysis.

Figure A.7: Business Cycle Turning Point Regimes
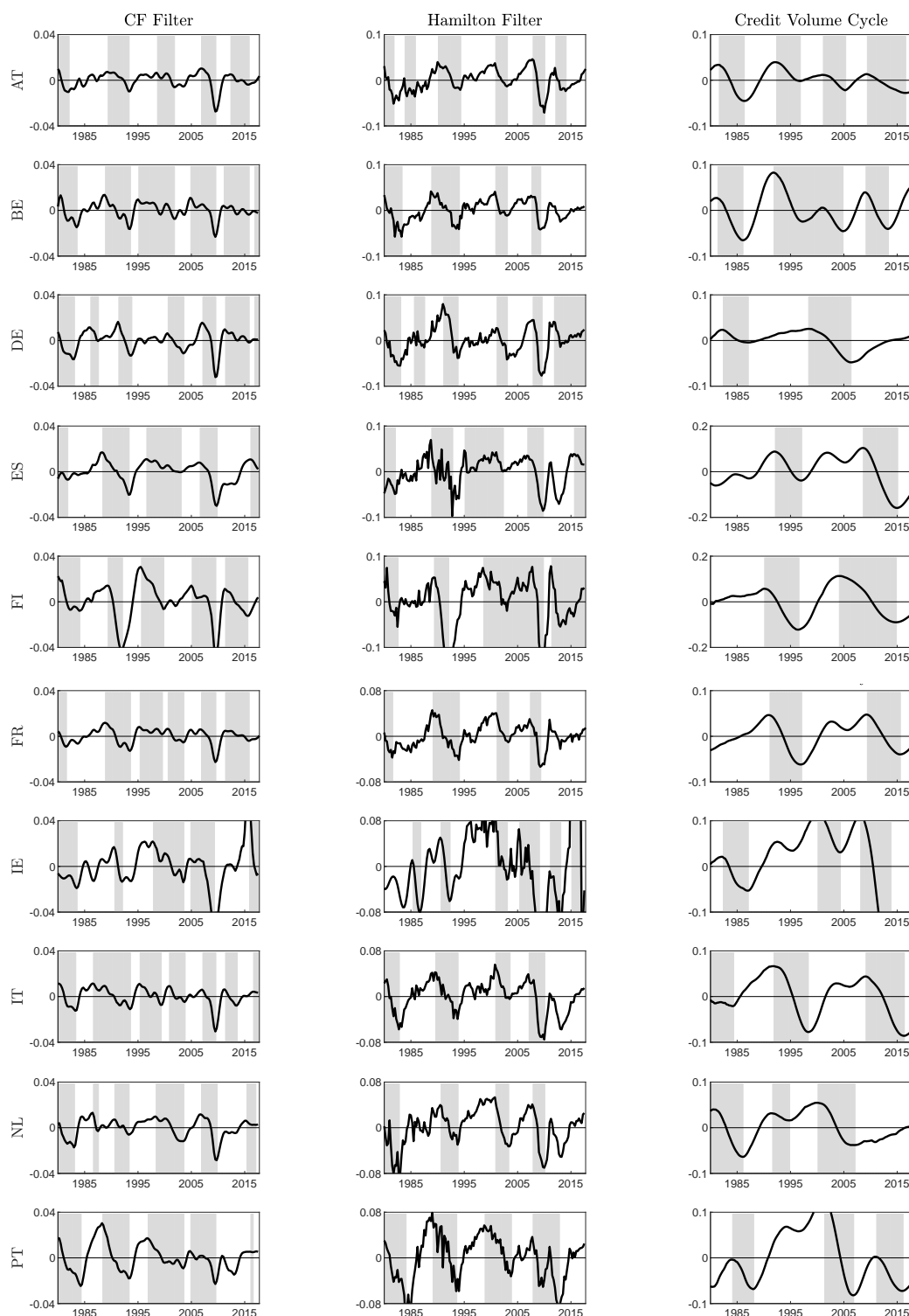
Estimates are based on the CF filter with a filter bands of 8-32 quarters and the Hamilton (2018) filter. For comparison purposes, the right column shows estimates of the credit cycle based on the *CF* filter (see Figure A.1). The shaded areas show the the contraction regimes based on turning point analysis.

Figure A.8: Synchronization of Cycles Across Countries: Bandpass Filter
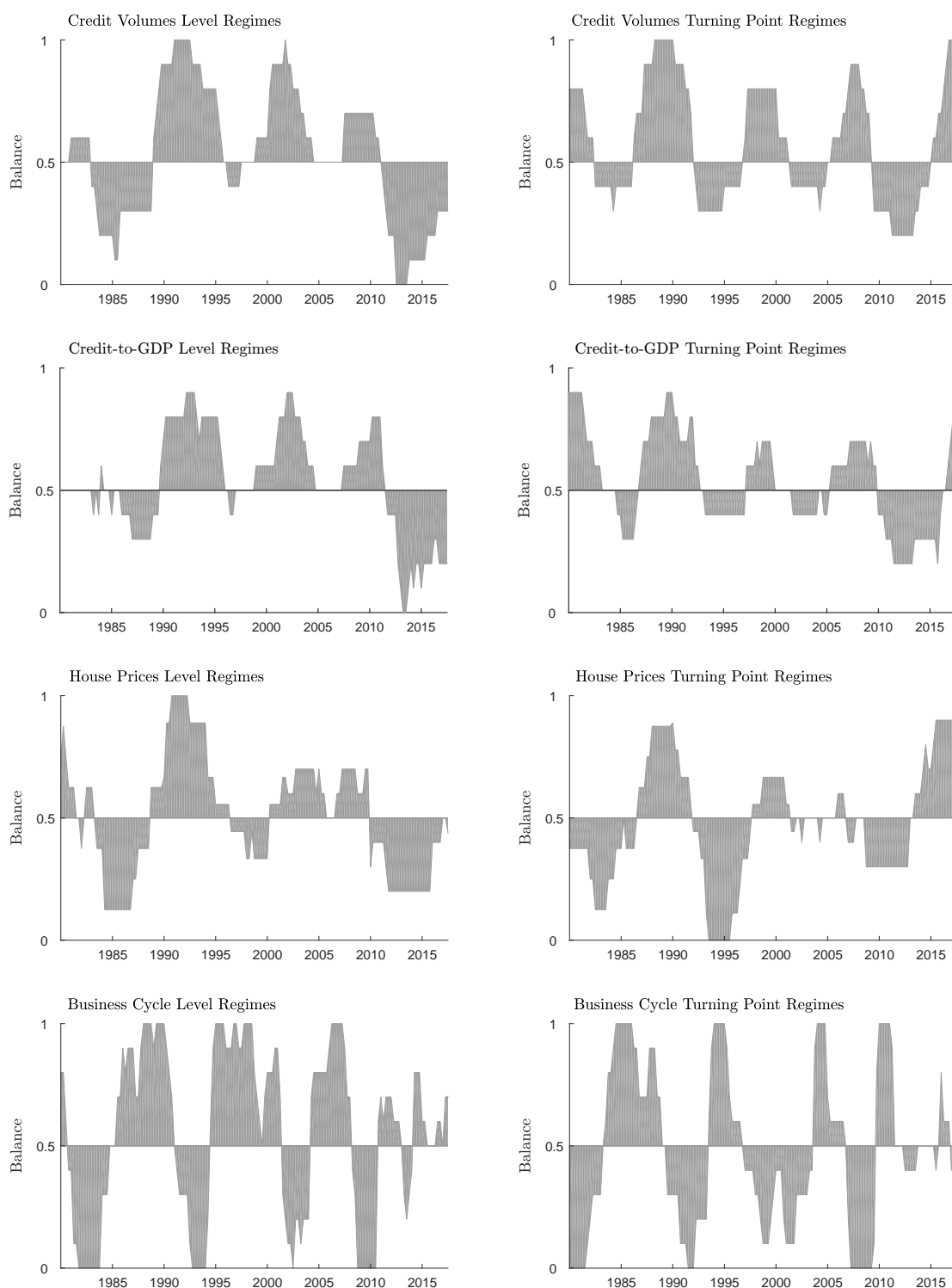
The left-hand graphs show the share of countries that face a high state in a given period. The right-hand graphs show the share of countries that face an expansion in a given period. The regimes are based on one-sided CF bandpass filters, as described in the main text. The graphs correspond to Figure 1 in the main text.

Figure A.9: Synchronization of Cycles Across Countries: Hamilton Filter
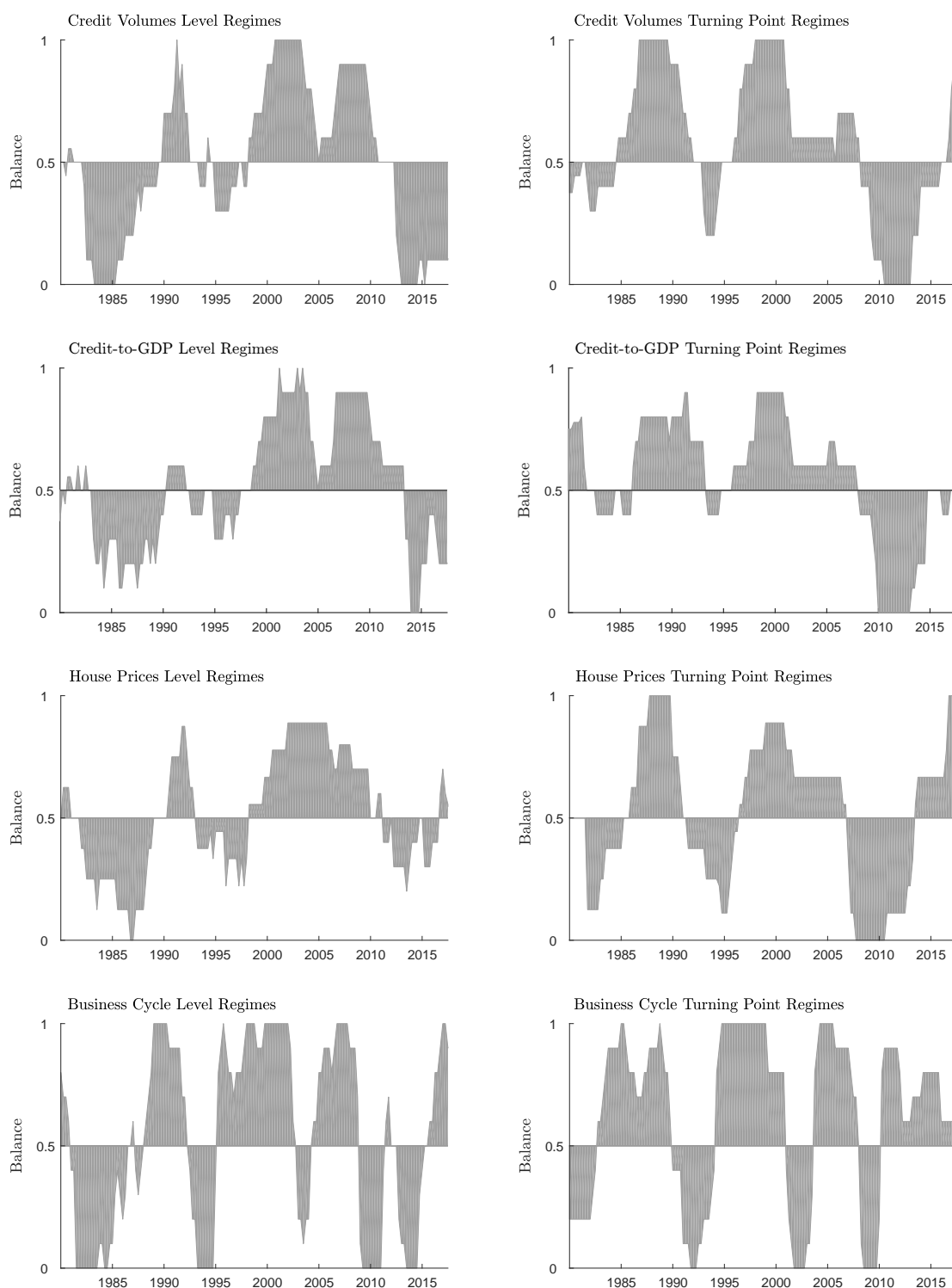
The left-hand graphs show the share of countries that face a high state in a given period. The right-hand graphs show the share of countries that face an expansion in a given period. The regimes are based on one-sided filter by Hamilton (2018) for the business cycle and its adaptation to credit and house price cycles by Drehmann and Yetman (2018).

Figure A.10: Synchronization of Cycles Across Countries: 3-Year Growth

Credit Volumes Level Regimes

House Prices Level Regimes

The graphs show the share of countries that face a high state in a given period. The regimes are based on the three-year growth rate in the series.

Table A.1: Cross Correlations Among Cycles (Filters)

	Credit Volumes			Credit-to-GDP			House Prices		
	CF RF	CF 3Y	RF 3Y	CF RF	CF 3Y	RF 3Y	CF RF	CF 3Y	RF 3Y
Min	.70	.57	.50	.65	.58	.50	.67	.38	.55
Median	.79	.67	.83	.76	.73	.83	.82	.61	.79
Max	.95	.89	.93	.94	.87	.90	.92	.72	.91
Max Cross-Corr	.83	.87	.89	.79	.82	.85	.84	.87	.90
Lag	-0.5	-5.0	-3.0	0.0	-5.0	-2.5	-1.0	-6.0	-3.0

Table A.2: Cross Correlations Among Cycles (Series)

	Bandpass Filter			Regression Filter			3-Year Growth		
	CC CY	CC HP	CY HP	CC CY	CC HP	CY HP	CC CY	CC HP	CY HP
Min	.84	.22	.05	.81	.24	.10	.85	.22	.03
Median	.94	.64	.45	.95	.71	.59	.95	.52	.41
Max	.98	.79	.76	.97	.85	.86	.98	.83	.70
Max Cross-Corr	.95	.77	.73	.95	.78	.65	.96	.64	.59
Lag	0.0	-1.5	-4.0	0.0	0.0	-3.0	0.0	-1.5	-3.0

The two tables show correlations among estimates of medium-term cycles over the period of 1982 Q1 to 2007 Q4. *CF*, *RF*, and *3Y* denote the bandpass filter, the regression filter, and 3-year growth rates, respectively. *CC*, *CY*, and *HP* denote credit volumes, the credit-to-GDP ratio and house prices, respectively.

The upper three rows show the minimum, median, and maximum value across countries of the contemporaneous correlations, the lower two rows show the medians of the maximum cross correlations and of the corresponding lags. A positive value of the lag stands for a lag of the series in the second row of the top panel.

Table A.3: Correlations of Business with Leverage Cycles

	Correlations Among Cycles				Correlations Among Level Regimes			
	Y-CF CC-CF	Y-CF HP-CF	Y-HF CC-HF	Y-HF HP-HF	Y-CF CC-CF	Y-CF HP-CF	Y-HF CC-HF	Y-HF HP-HF
Min	-.32	-.42	-.10	-.23	-.21	-.33	-.01	-.27
Median	-.23	-.20	.34	.46	-.04	-.10	.24	.21
Max	.27	.09	.58	.69	.18	.28	.58	.52

The left-hand panel shows the minimum, maximum and median values across countries of the correlations among estimates of cycles from 1982 Q1 to 2007 Q4. The right-hand panel shows the correlations between the resulting level regimes. *C* and *HP* denote cycles in credit volumes and real house prices, *CF* and *HF* denote bandpass and regression filters, respectively.

Table A.4: Properties of GDP and Inflation (Level Regimes)

	CC-CF		CC-RF		HP-CF		HP-RF	
	High	Low	High	Low	High	Low	High	Low
Inflation level	3.27	4.32	2.95	4.62	3.66	3.38	3.26	3.60
Inflation volatility	1.55	3.17	1.52	3.18	2.24	2.35	1.73	2.48
GDP growth	.64	.73	.69	.65	.63	.70	.65	.67
GDP volatility	.91	.99	.84	1.02	.92	1.02	.85	1.01

The table shows the average across countries of statistics for quarterly GDP growth and annual inflation for level regimes over the period of 1982 Q1 to 2007 Q4.

Table A.5: Properties of GDP and Inflation (Turning Point Regimes)

	CC-CF		CC-RF		HP-CF		HP-RF	
	E	C	E	C	E	C	E	C
Inflation level	3.62	3.96	3.16	4.84	3.21	4.01	2.89	4.08
Inflation volatility	2.33	2.50	1.84	3.25	1.98	2.59	1.56	2.52
GDP growth	.81	.48	.83	.41	.80	.53	.82	.41
GDP volatility	.90	.99	.90	.94	.95	.97	.92	.95

The table shows the average across countries of statistics for quarterly GDP growth and annual inflation for turning point regimes over the period of 1982 Q1 to 2007 Q4. *E* and *C* denote expansion and contraction regimes, respectively.

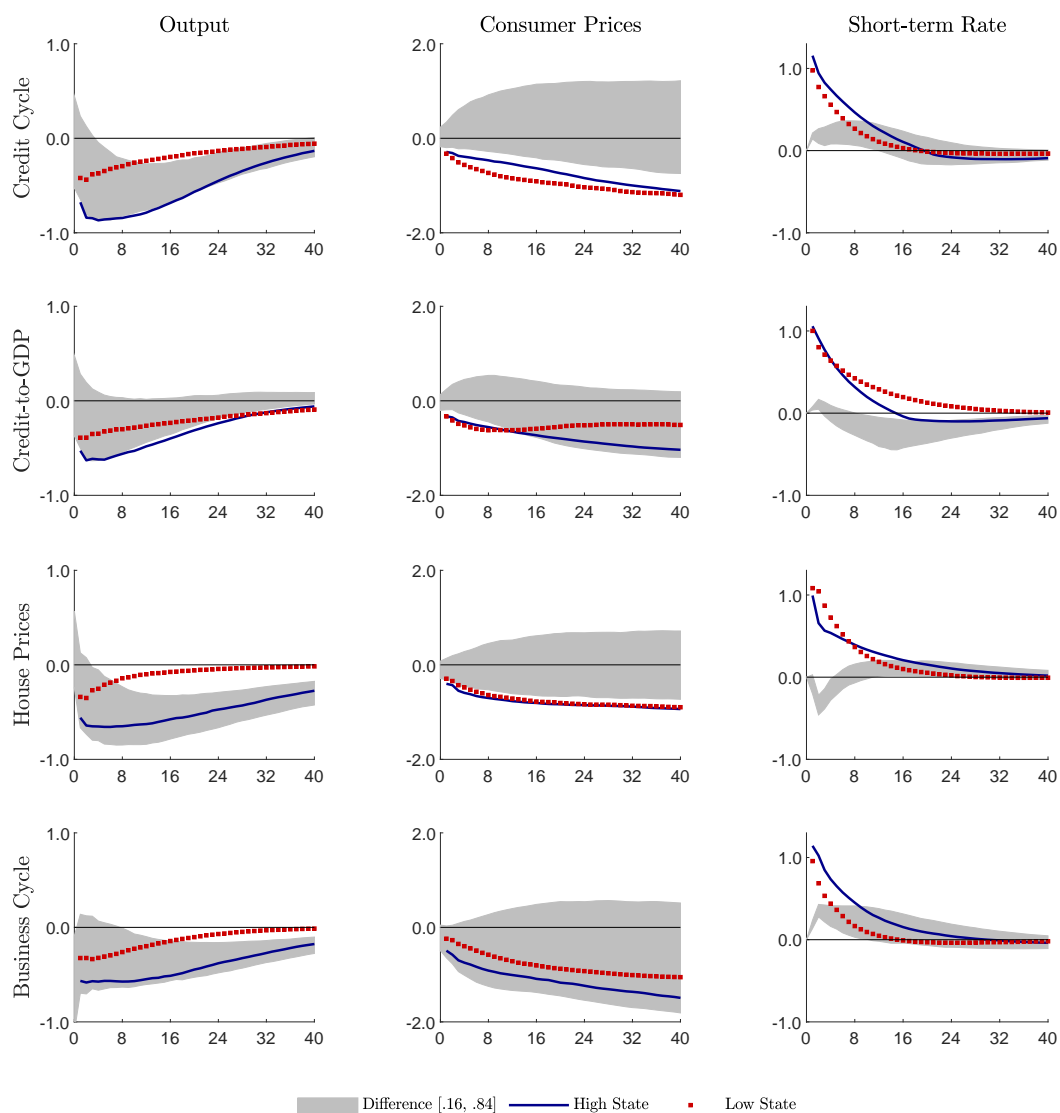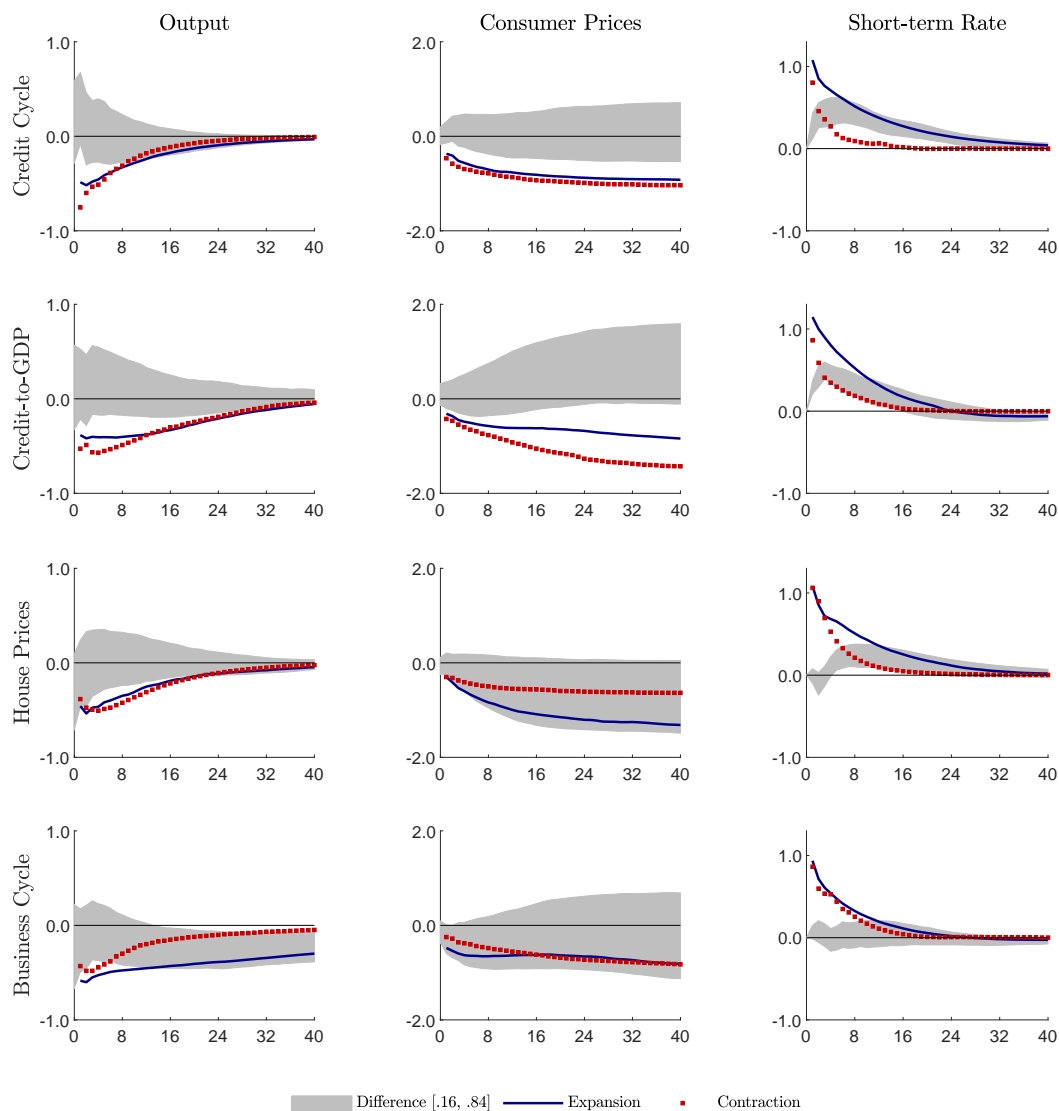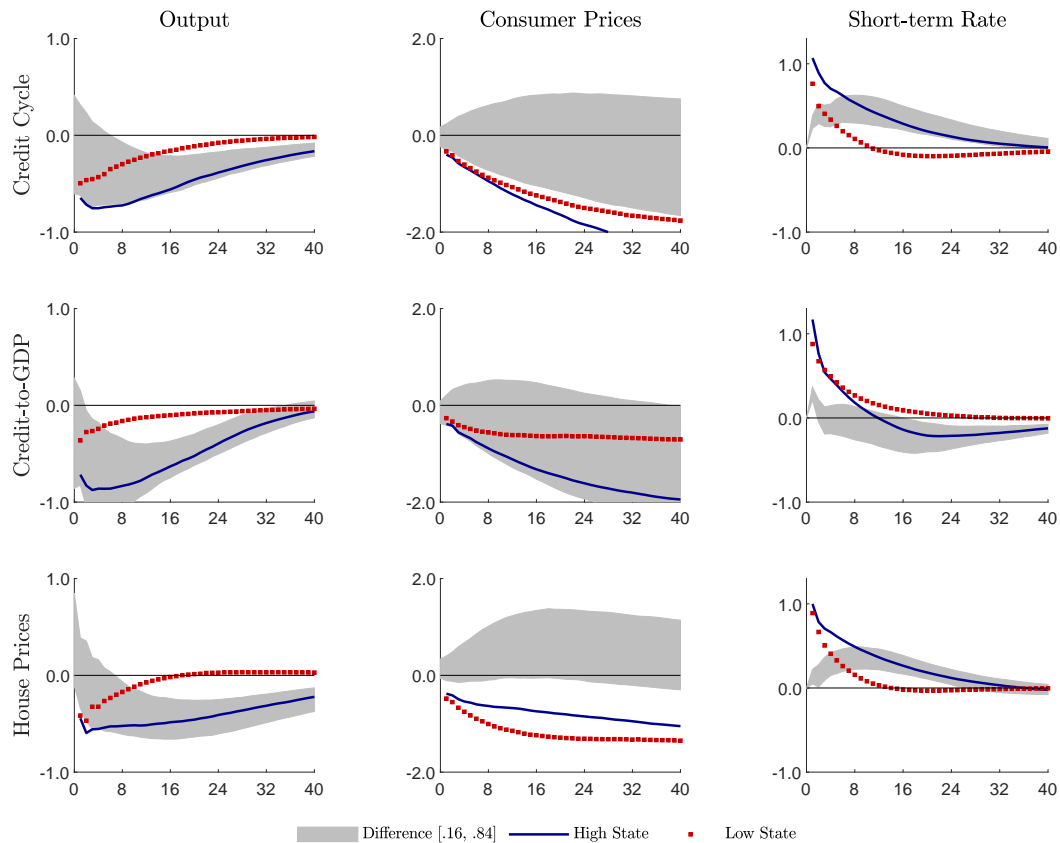## Figure A.11: IRFs from linear VAR
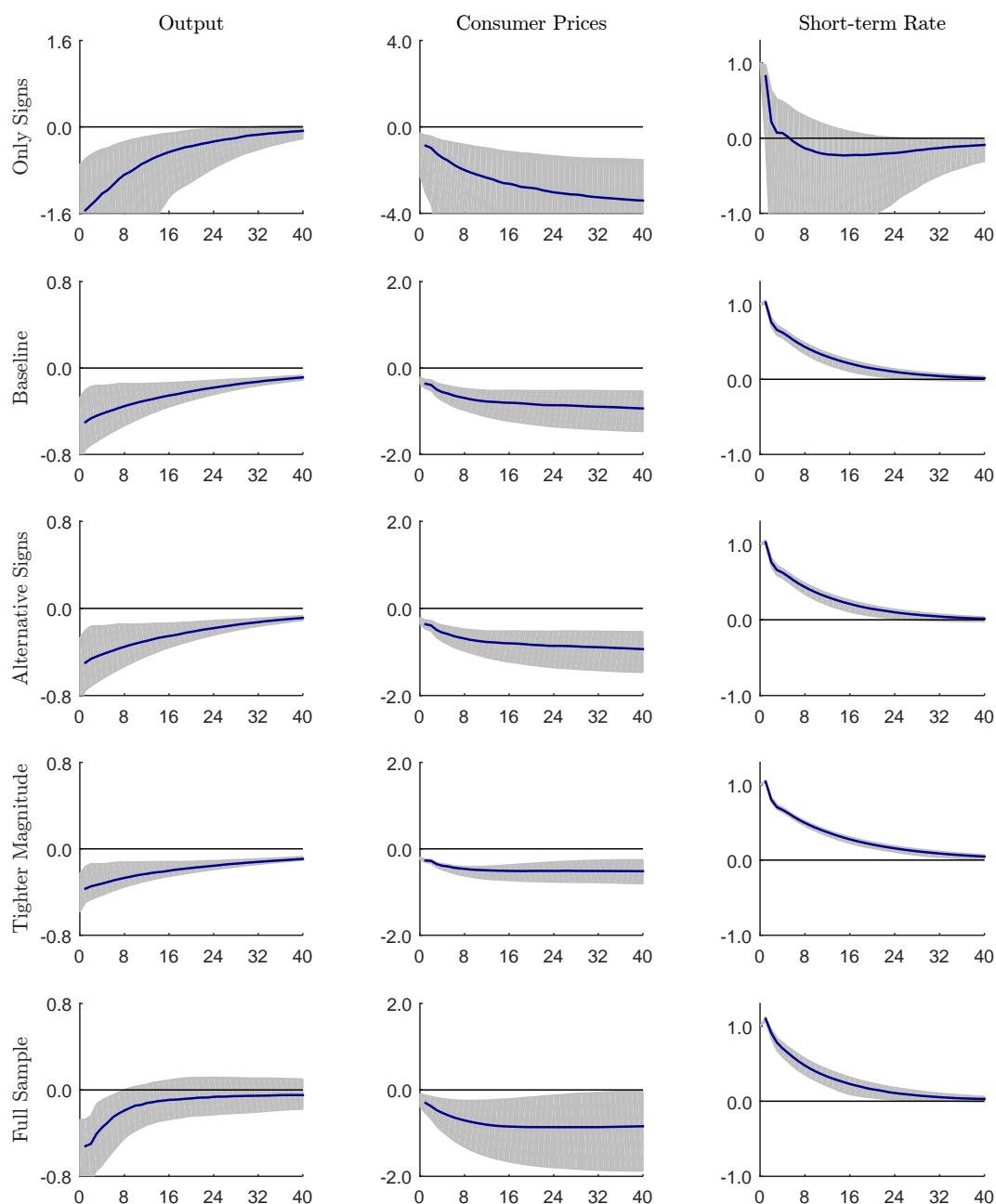
The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the linear VAR. Shaded areas show the [.16;.84] credible set for the estimates. Rows 1 to 4 show the results for the sample from 1982 Q1 to 2007 Q4 for different combinations of sign and magnitude restrictions. Row 1 uses only the baseline sign restrictions described in section 3.1. Row 2 adds a magnitude threshold of 33% (baseline model). Row 3 employs sign restrictions only at a horizon of 2 quarters, while Row 4 adds a tighter threshold of 20%. Row 5 shows results for the baseline model estimated over the full sample from 1982 Q1 to 2017 Q4. Rows 1 and 2 correspond to Figure 2 in the main text.

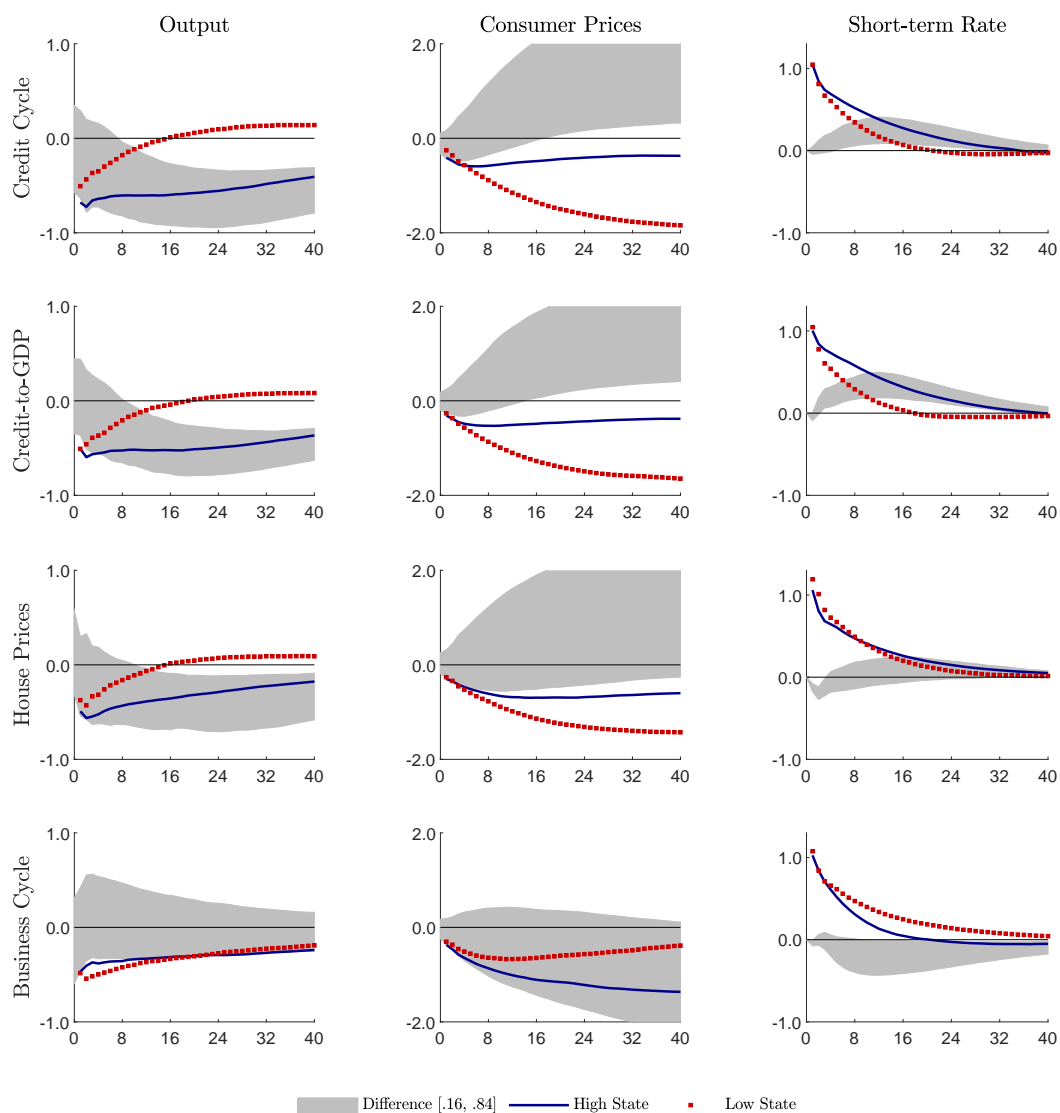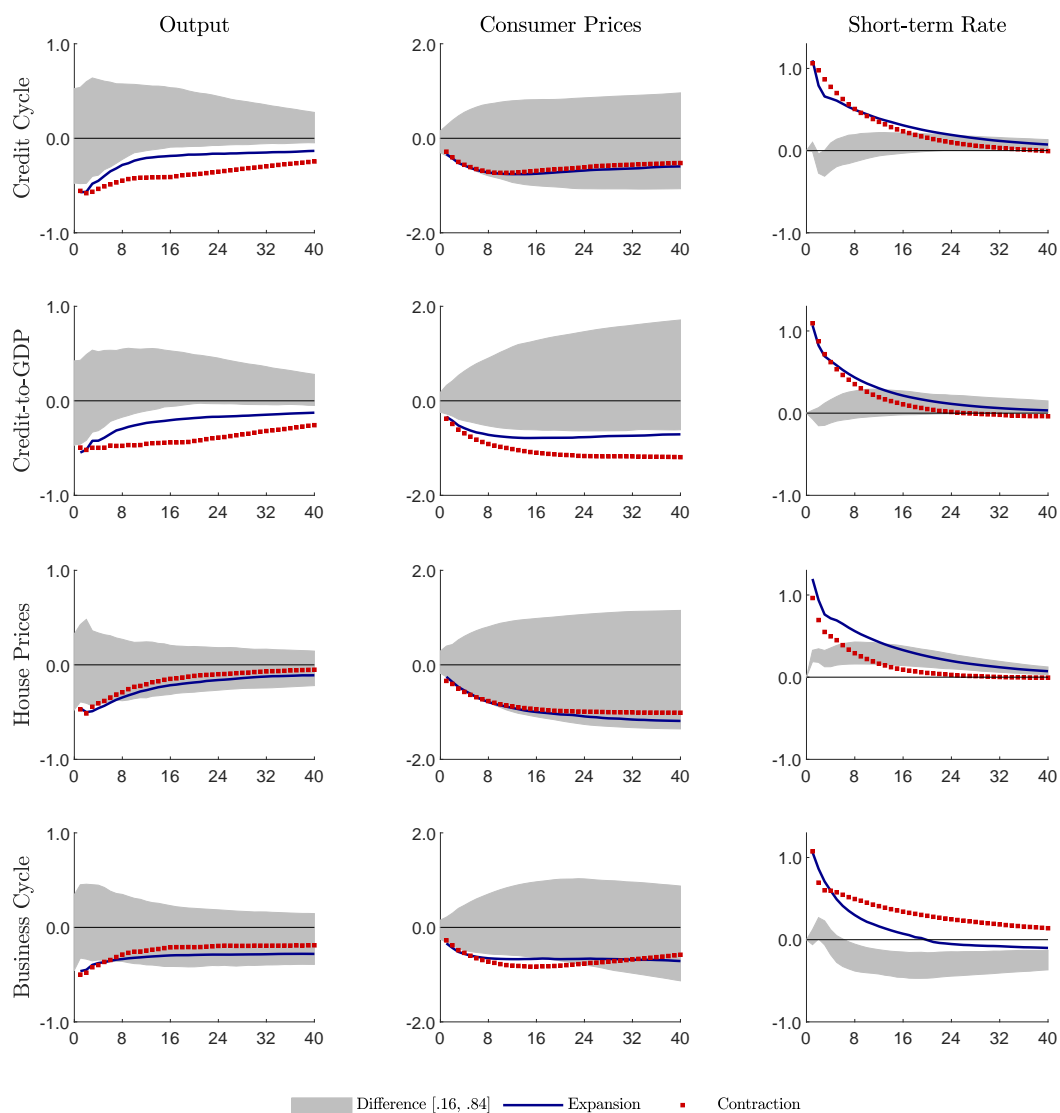## Figure A.12: Bandpass Filters Level Regimes
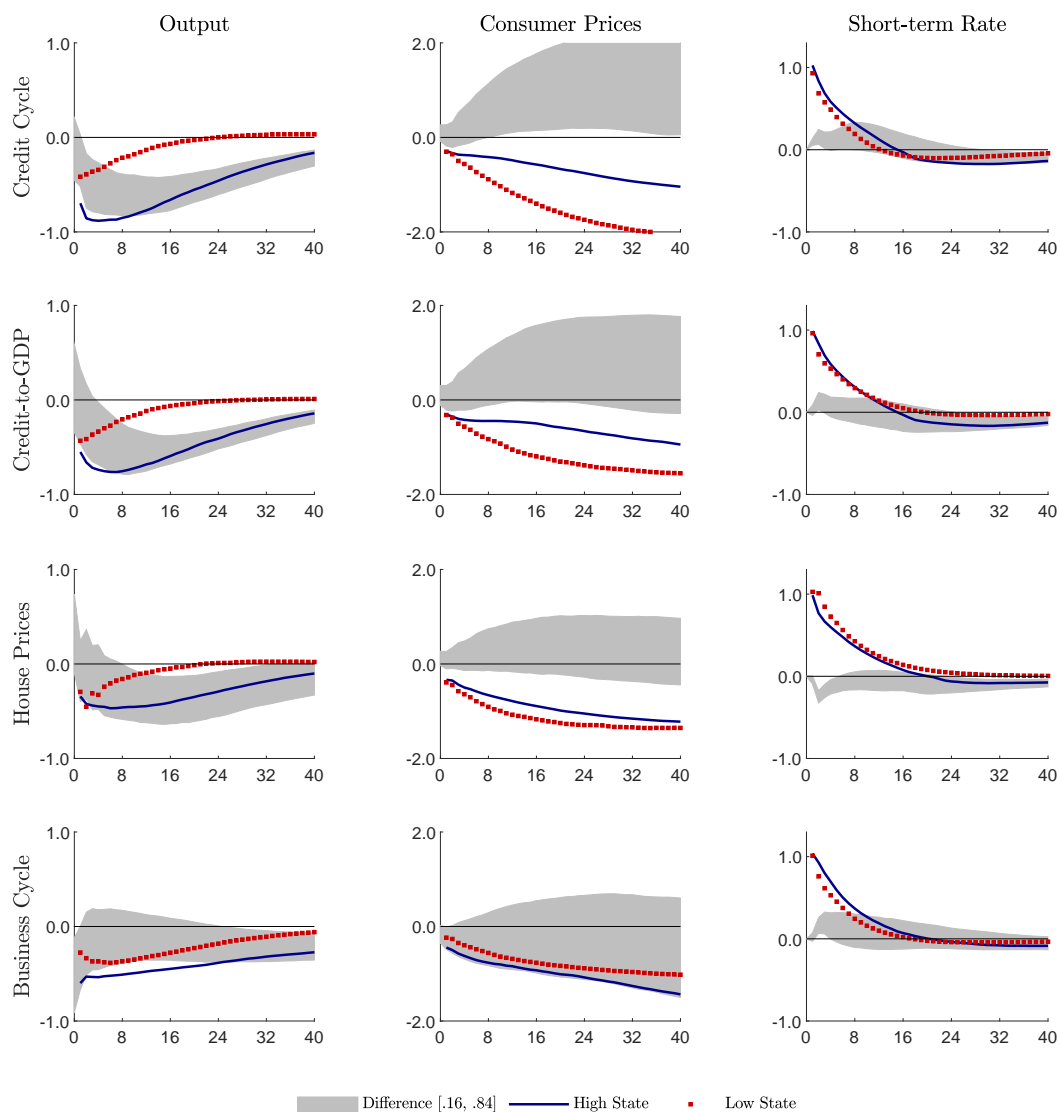
The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. With the exception of the credit-to-GDP cycle the plots correspond to those in Figure 3 in the main text. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.
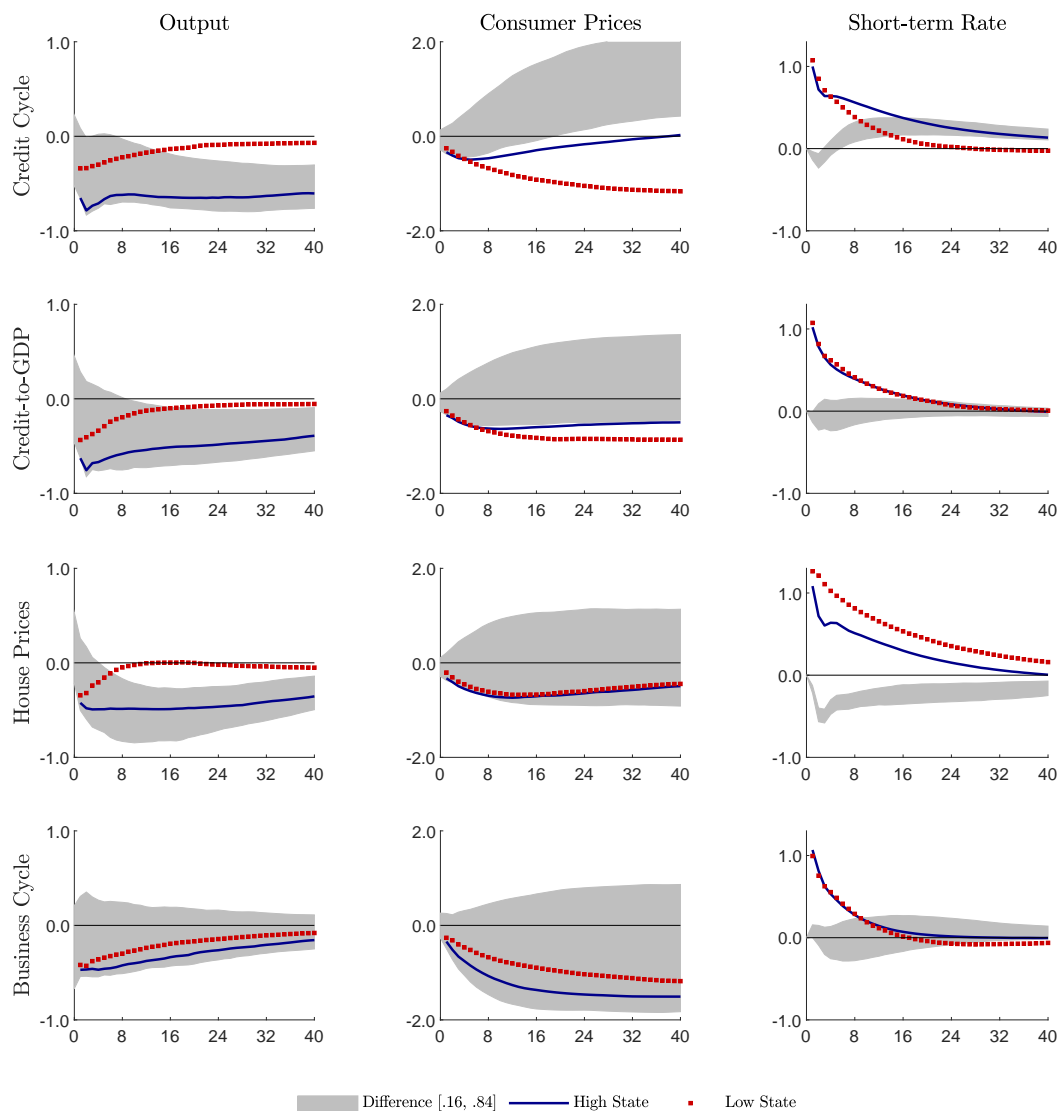
Figure A.13: Bandpass Filters Turning Point Regimes
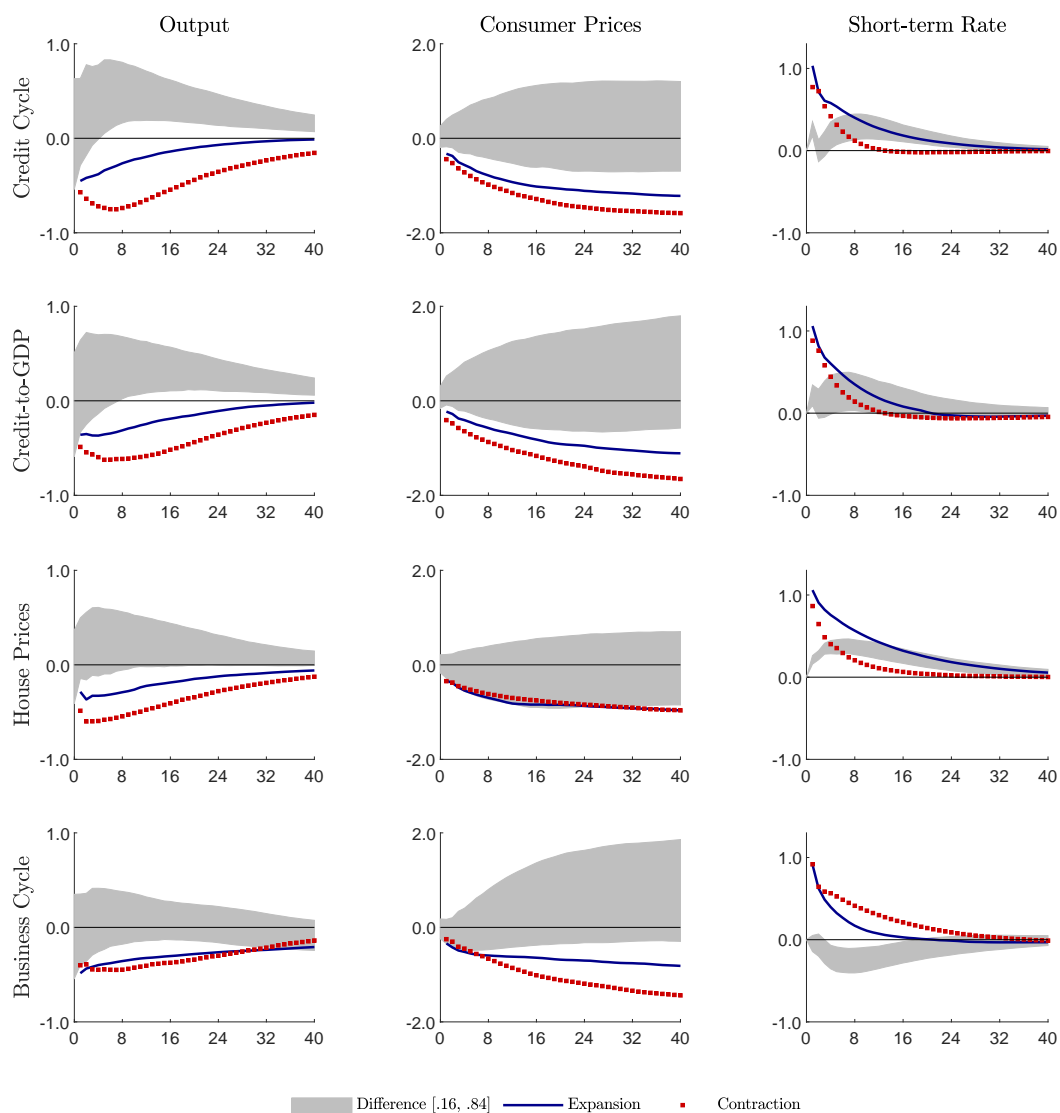
The plots show the IRFs to a monetray policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.14: Regression Filters Level Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. Regimes are based on the filter by Drehmann and Yetman (2018) and by Hamilton (2018) for business cycles as described in section 4.3. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.
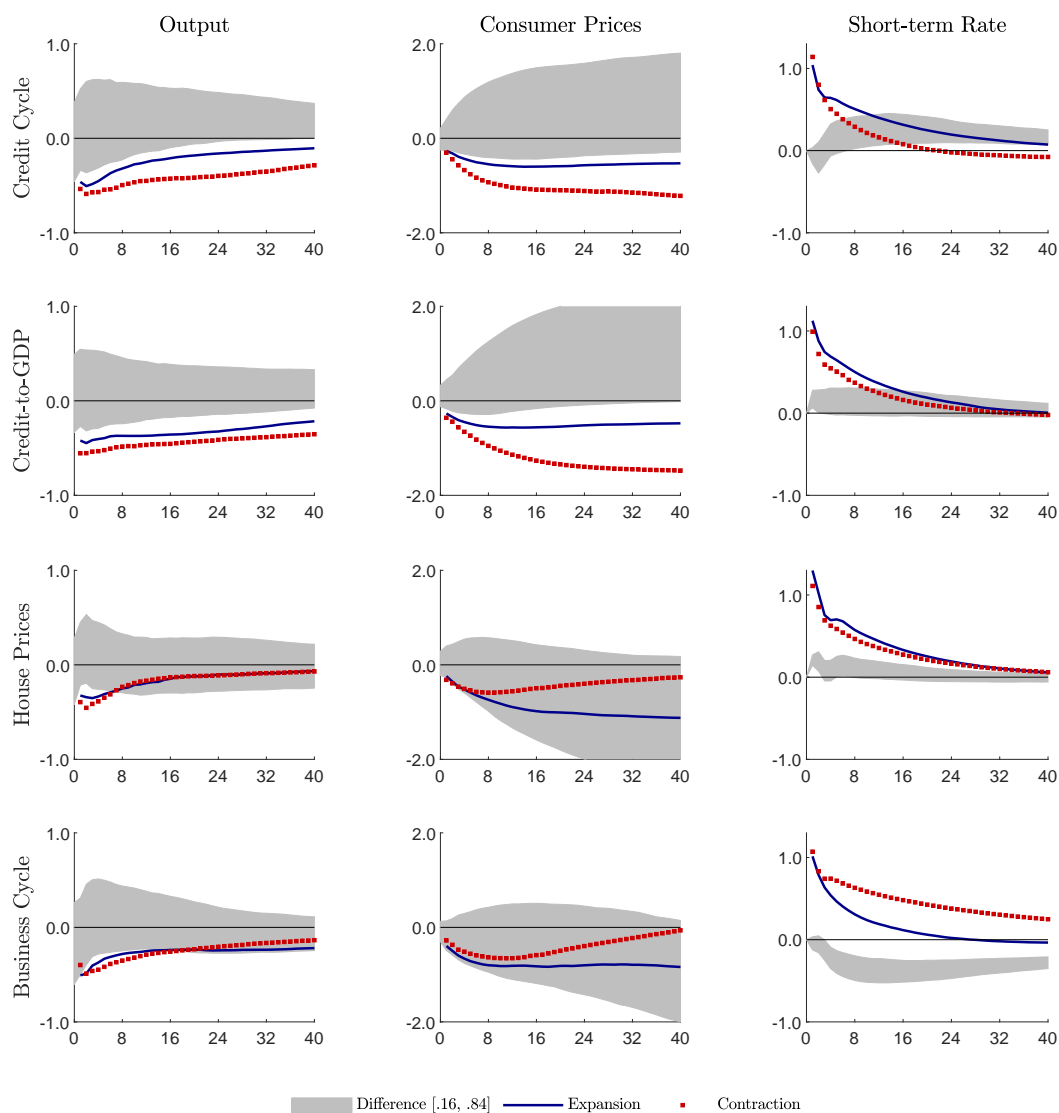
Figure A.15: Regression Filters Turning Point Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. Regimes are based on the filter by Drehmann and Yetman (2018) and by Hamilton (2018) for business cycles as described in section 4.3. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.
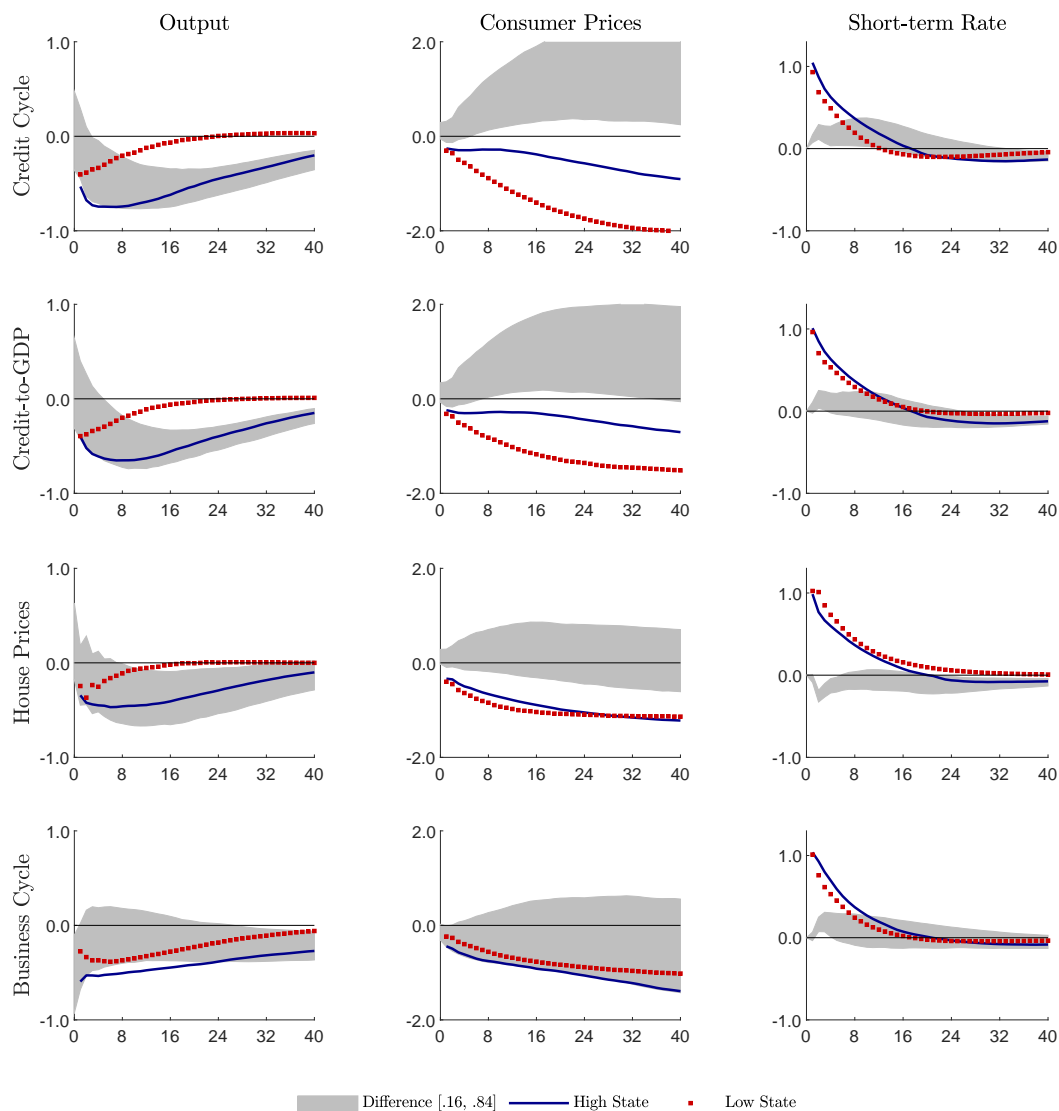
Figure A.16: Three Year Growth Rate Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. Regimes are based on the three-year growth rate in the series. The high state is defined as the growth rate being above its country mean. Blue solid and red dotted lines show the effects in high (growth above country mean) and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.17: Full Sample Estimates Bandpass Filter Level Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The estimation sample ranges from 1982 Q1 to 2017 Q4. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes.
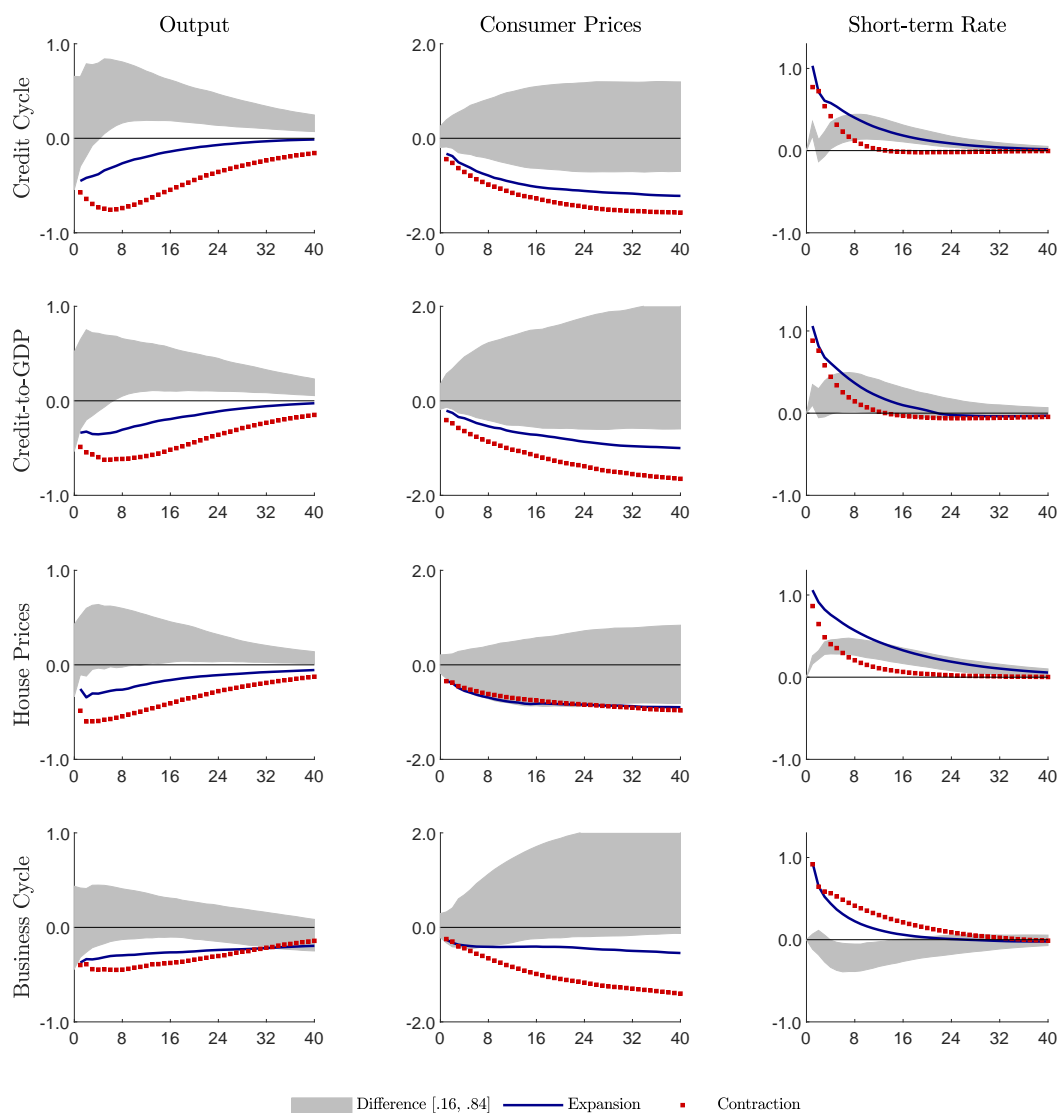
Figure A.18: Full Sample Estimates Bandpass Filter Turning Point Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The estimation sample ranges from 1982 Q1 to 2017 Q4. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes.

Figure A.19: Full Sample Estimates Regression Filter Level Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The estimation sample ranges from 1982 Q1 to 2017 Q4. Regimes are based on the filter by Drehmann and Yetman (2018) and by Hamilton (2018) for business cycles as described in section 4.3. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes.

Figure A.20: Full Sample Estimates Regression Filter Turning Point Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The estimation sample ranges from 1982 Q1 to 2017 Q4. Regimes are based on the filter by Drehmann and Yetman (2018) and by Hamilton (2018) for business cycles as described in section 4.3. Blue solid and red dotted lines show the effects in in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes.

Figure A.21: Sign Restrictions at 2 Quarters Level Regimes

The plots show the results for the baseline VAR with sign restrictions on output and inflation imposed at a horizons of 2 quarters instead of 2 to 4 quarters as in the baseline model. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycle. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.22: Sign Restrictions at 2 Quarters Turning Point Regimes

The plots show the results for the baseline VAR with sign restrictions on output and inflation imposed at a horizons of 2 quarters instead of 2 to 4 quarters as in the baseline model. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

## Figure A.23: Tighter Magnitude Restrictions Level Regimes
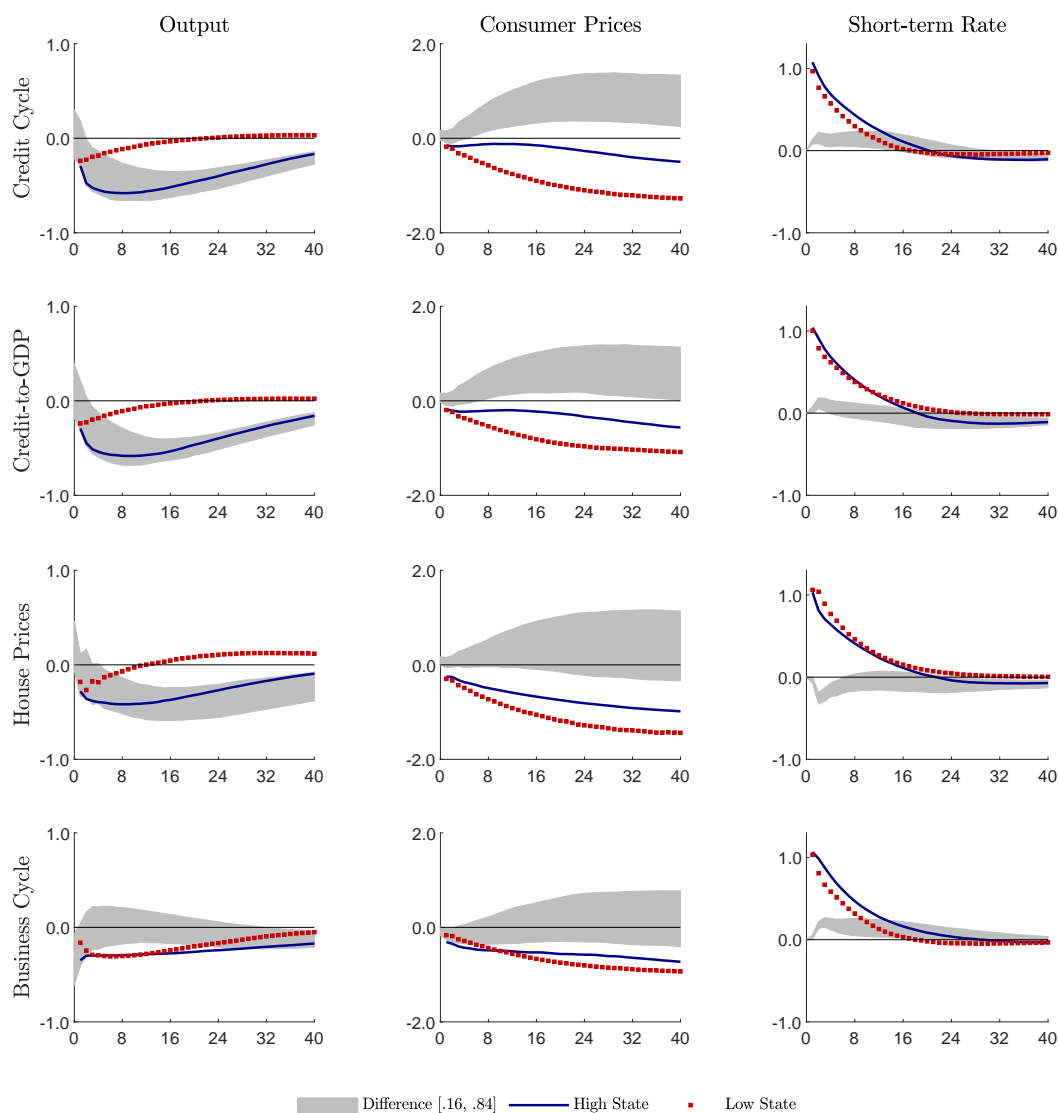
The plots show the results for the baseline model with the threshold for the contributions of monetary policy shocks to the one-step ahead forecast errors of output and inflation set to 20% as compared to 33% in the baseline VAR. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycle. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.24: Tighter Magnitude Restrictions Turning Point Regimes

The plots show the results for the baseline model with the threshold for the contributions of monetary policy shocks to the one-step ahead forecast errors of output and inflation set to 20% as compared to 33% in the baseline model. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

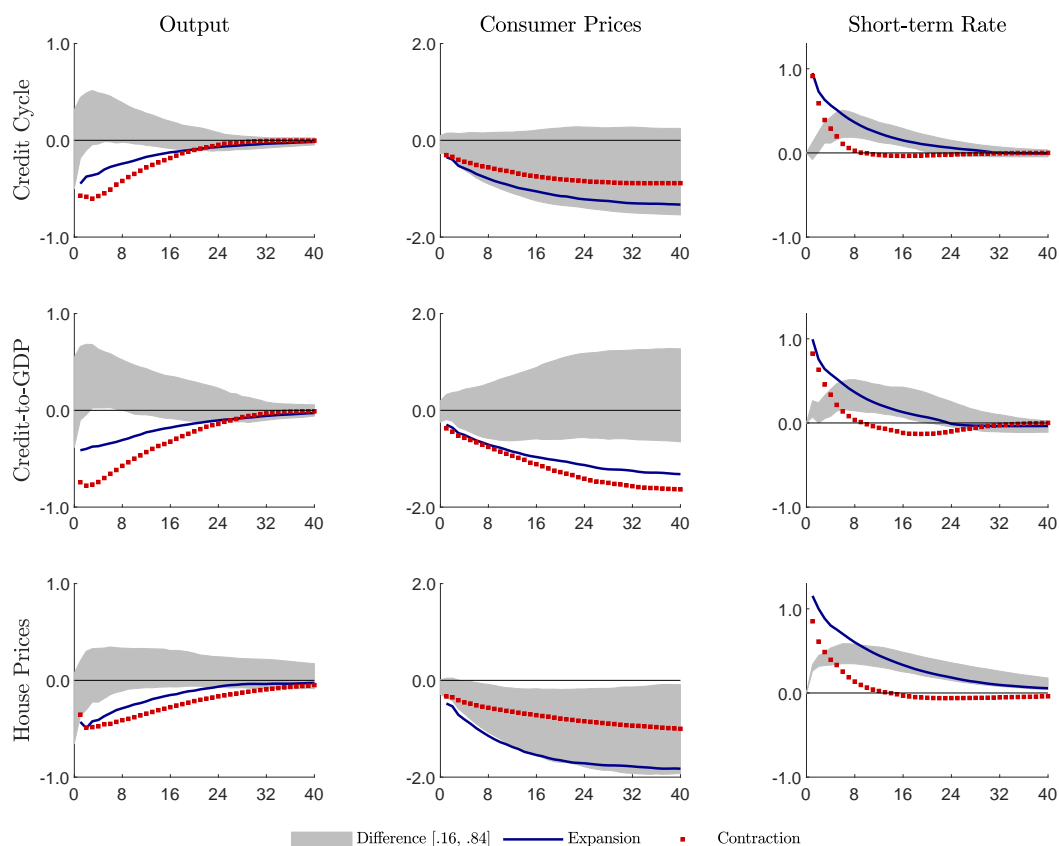## Figure A.25: Extended Predetermined Variables Level Regimes
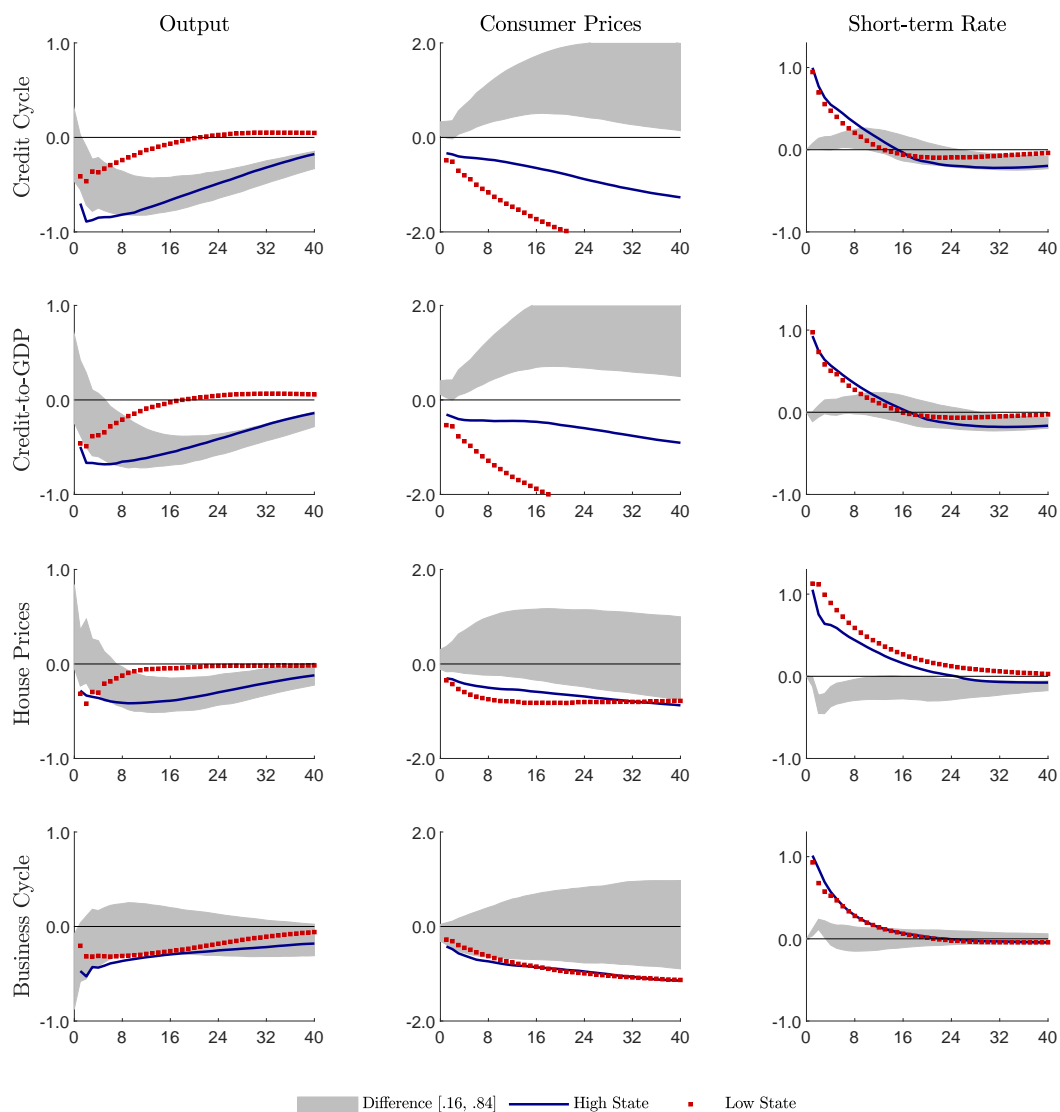
The plots show the results for the baseline model with predetermined variables added at lags 1 to 3 instead of only at lag 1 as in the baseline model. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycle. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.26: Extended Predetermined Variables Turning Point Regimes
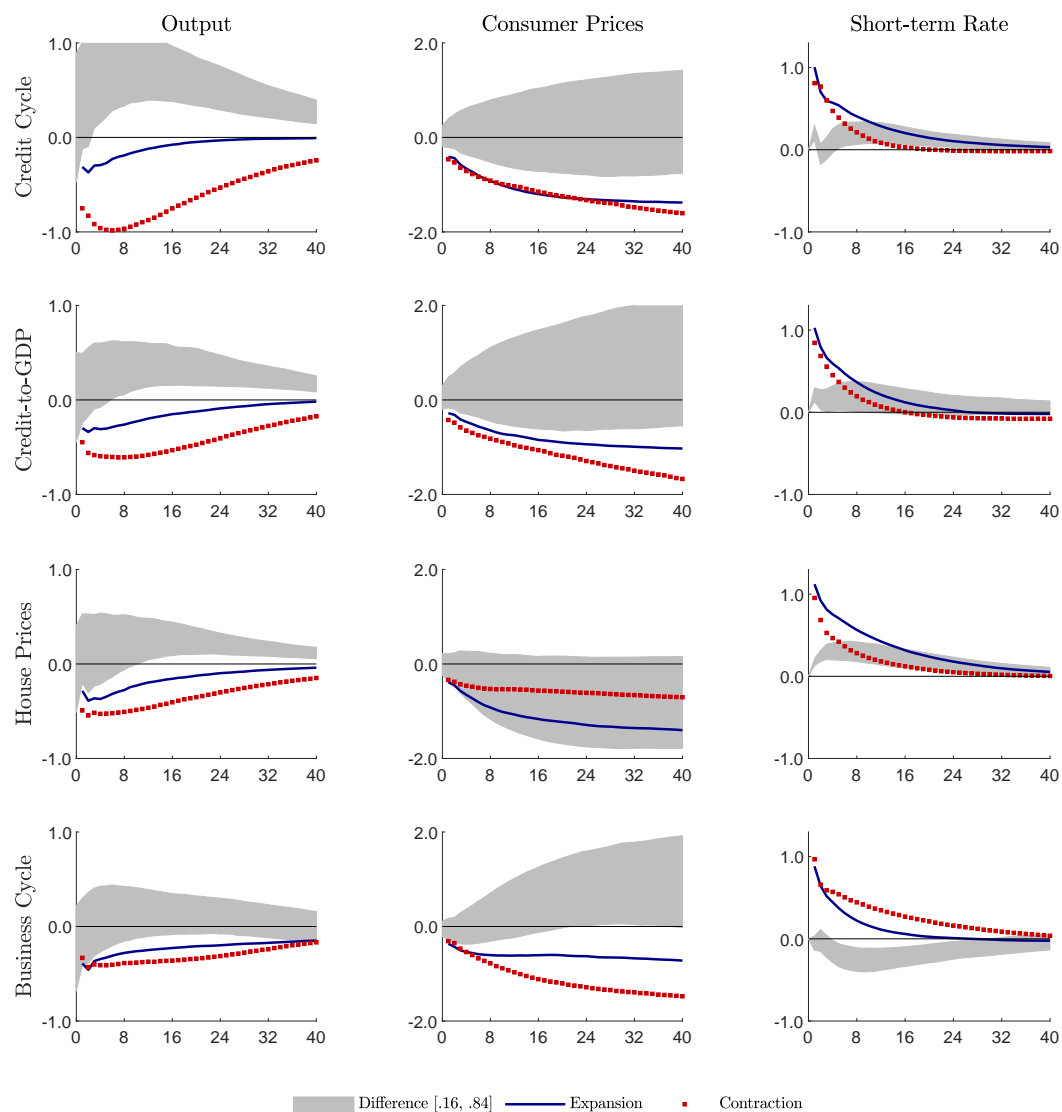
The plots show the results for the baseline model with predetermined variables added at lags 1 to 3 instead of only at lag 1 as in the baseline model. They show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in expansion and contraction regimes. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.27: Bandpass Filter (h=1) Level Regimes
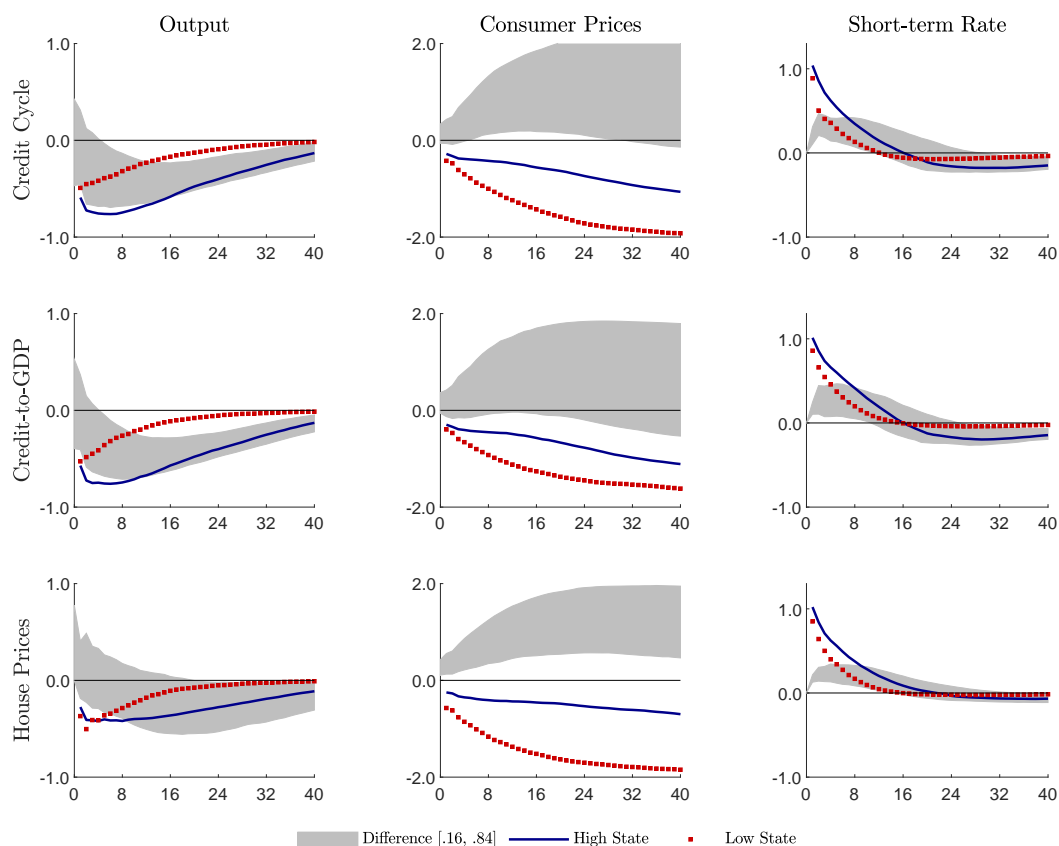
The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The VAR uses state variable $s_{c,t-h}$ at a lag of $h = 1$ instead of $h = 5$ as in the baseline model. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

Figure A.28: Bandpass Filter (h=1) Turning Point Regimes

The plots show the IRFs to a monetary policy shock scaled to an increase in the policy rate by 100 basis points for the baseline VAR. The VAR uses state variable $s_{c,t-h}$ at a lag of $h = 1$ instead of $h = 5$ as in the baseline model. Regimes are based on the CF filter with a filter band of 32 to 80 quarters and of 8 to 32 quarters for business cycles. Blue solid and red dotted lines show the effects in high and low states of the cycles. The shaded areas show [.16; .84] credible sets for the differences between regimes. The estimation sample ranges from 1982 Q1 to 2007 Q4.

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